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ABSTRACT

This monograph analyzes transition in educational administrative theory. A brief introductory section describes the theoretical movement, the substance and repercussions of Thomas Greenfield's critique of educational administrative theory in 1974, and emerging qualitative approaches. Seven readings, all written by the volume's author, view administrative theory from a variety of past, present, future, ideological, and behavioral perspectives. The readings are: (1) "Theory in Educational Administration: 1966"; (2) "Another Look at Research on the Behavior of Administrators"; (3) "Theories: Past, Present, and Future"; (4) "Evolution in Research and Theory: A Study of Prominent Researchers"; (5) "Can There Be a Science of Organisations?"; (6) "The Individual in Organization: A Theoretical Perspective"; and (7) "Intellectual Turmoil in Educational Administration." A list of references concludes each reading; an annotated bibliography is appended. (CJH)

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ESA841 THEORY AND PRACTICE IN EDUCATIONAL ADMINISTRATION

ADMINISTRATIVE THEORY IN TRANSITION

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Administrative theory in transition

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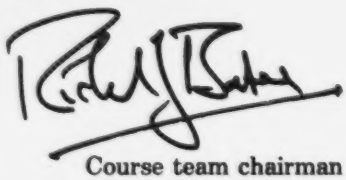
Series Introduction

It is now widely recognised, among theorists and practitioners alike, that the traditions that have informed educational administration as a field of study for several decades are of only limited use in coming to terms with the complexity and value-laden nature of educational practice. The sudden politicisation of the context and conduct of education has raised issues of immediate import that cannot be dealt with adequately by functionalist analysis or behaviourist science. The collapse of these theoretical traditions in educational administration has produced a vacuum into which a very haphazard collection of intellectual bric-a-brac has been sucked. As a result, both theorists and the practitioners who look to them for help in an increasingly disordered world are alike in their bewilderment. How can alternative formulations be developed? How can reliable and relevant analyses be made?

The series of books of which this volume is a part is an attempt to explore a variety of intellectual traditions that have, until now, been largely ignored or dismissed by educational administrators. Each of the books is an attempt to bring a particular intellectual perspective to bear on the practical problems of administering education. They are, therefore, diverse in their starting points and in their analysis. What they have in common, however, is a rejection of a purely technical, functionalist approach to educational administration, and a commitment to a critical and reflexive consideration of educational practice.

The ideas presented in the introductory essays are necessarily an encapsulation of arguments that have developed and are developing more fully elsewhere. In order to assist readers to participate in these developments, selected readings are attached to each paper, and an annotated bibliography of key works is provided. We hope that the publication of this series will encourage others to join a necessary exploration of alternative perspectives in educational administration. Such exploration is long overdue.



Course team chairman

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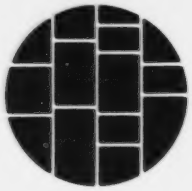
Administrative theory in transition



To almost every literate person in the world, *1984* is the title of George Orwell's attempt to prophesy the future, but to those of us in educational administration there is an added significance to the year. Nineteen eighty-four marks the thirtieth anniversary of the birth of the 'theory movement' and the tenth anniversary of its seminal criticism by Greenfield. I was present at both events and both have left on me indelible marks.

The movement was 'born' at the National Conference for Professors of Educational Administration (NCPEA) annual meeting held in Denver, Colorado in August, 1954. The criticism was aired at the International Intervisitation Programme (IIP) quadrennial meeting held in Bristol, England, in 1974. Both were marked by strong emotions: excitement and anticipation at Denver; a mixture of disbelief, anger and rejection on the part of some, and revelation, delight and acceptance on the part of others at Bristol. It is my opinion that, while the active portion of the theory movement was short-lived (probably only ten years), it had a lasting effect on the literature of the field.

The theory movement

The events surrounding the birth and relatively short life of the theory movement are beginning to be shrouded in mist. Most of those present at the birth are either retired or dead. Much of the present-day discussion of the theory movement is carried on by those whose sources of knowledge are either hearsay or the sparse written accounts and articles and books of the period. There has been no definitive history of the period, but it appears that this shortcoming is in the process of being remedied. While not present at the birth, Culbertson became deeply involved with the movement shortly afterwards, and has now begun an intensive study of the period. With his background in philosophy and administration, and his personal acquaintance with the chief actors of the movement, he is uniquely qualified to do its history. His first article (I hope, of a long line) was published in 1983. He attempted to trace the antecedents of the theory movement and to analyse the work of its critics. He identified the following as the core ideas:

1 Statements about what administrators and organizations ought to do cannot be encompassed in science or theory.
2 Scientific theories treat phenomena as they are.
3 Effective research has its origins in theory and is guided by theory.
4 Hypothetico-deductive systems are the best exemplars of theory.
5 The use of the social sciences is essential in theory development and training.
6 Administration is best viewed as a generic concept applicable to all types of organization (Culbertson 1983, p. 15).

These ideas, according to Culbertson, were best presented in Halpin's (1958) report of the first University Council for Educational Administration (UCEA) seminar held at the University of Chicago in 1957, although they were earlier revealed to the NCPEA at Denver. Culbertson credited the first five ideas to positivism and indicated that they came into the literature of administration by way of Simon's *Administrative Behavior* (1945).

My personal recollection of the period leads me to agree with Culbertson, although I believe that the notion of operationalism should be added to Culbertson's list. Halpin, in particular, made a great deal of Bridgman's (1927) concept of operationalism, contending that it was the antidote to fuzzy concepts, one of the generally accepted weaknesses in the literature of educational administration. I supported Halpin fully in his stand on operationalism. Inasmuch as operationalism had a significant impact on research into educational administration, it is surprising that Culbertson omitted it from his list.

In retrospect, one might wonder why the ideas listed by Culbertson plus the concept of operationalism would make such an impact on professors of educational administration. There were probably many reasons, but most likely the main reason was the *context* of educational administration in the early 1950s. The Kellogg Foundation had become interested in the improvement of education in the United States of America, and, in line with their basic philosophy, decided to improve the leadership of American public education. In 1950 the Foundation funded the Cooperative Program in Educational Administration (CPEA) by setting up eight university centres, one for each region of the country. The grant totalled some four and a half million dollars for four years (an unheard of amount of money for educational administration). The grant was extended for another four years and the total amount was some eight million dollars.

The NCPEA was founded in 1947 and devoted its efforts towards improving the education of school administrators.

There was, in addition, a considerable amount of activity in disciplines other than education. Among the various universities and research centres engaged in inquiry into administration were the Graduate School of Business Administration at Harvard, the Yale Labor and Management Center, the Research Center for Group Dynamics at the University of Michigan, and the Personnel Research Board at Ohio State University. These centres were making contributions to research and theory which changed the nature of the literature of administration. The early 1950s, then, was a period in which the context of administration was one of excitement, of new ideas, and of support on the greatest scale ever.

But there were other factors. Several of the CPEA centres brought in social scientists, generally as consultants, to advise on theory, research, and, in some cases, training. These centres sent several social scientists to Denver for the 1954 NCPEA meeting. They were highly articulate, in many cases charismatic individuals who had a message to give: better research into educational administration was needed; the research must be theory based; social science was the source of the theories; and the social scientists were the ones who could guide the professors of educational administration. Most prominent among the social scientists were Jack Getzels (University of Chicago), Andrew Halpin (Ohio State University), and Arthur Coladarci (Stanford University). They were extremely effective in spreading 'the word', converting virtually everyone in attendance.

The last element in the mixture which brought about the theory movement was the professors of educational administration. First, there was a group of young professors who, while extremely interested in administration, were completely disillusioned with the literature of the field, with the lack of a knowledge base, and with the intellectual drabness of the

subject. They were prepared to take a leadership role. The remainder of the professors seemed to be very much aware of their lack of social science sophistication, research ability and theory know-how. They were ready to be led.

This, then, was the context in which the theory movement was born. What of the Greenfield critique?

The Greenfield critique

The Greenfield critique took place in Bristol, England, on a hot July afternoon in 1974. It followed, by a day or two, a very provocative paper by Lord Morris. The Morris paper was a surprise since presumably he came to read a greeting from the Queen and 'show the flag'. He did that, but then took the opportunity to present in a nutshell the case for a new kind of administrator (see Griffiths 1977). In the United States of America we had been talking about a new kind of administrator, but we had not been able to decide what was new. I felt that what Lord Morris said was a highly significant insight and it certainly shook me up. That was on the Monday morning. Then I read the Greenfield paper and was disturbed by certain of the allegations, among other reasons. I was indignant, for instance, at Greenfield's condemnation of Chester Barnard *and* of his theoretical work on the ground that he had once sworn at a group of workers. Almost everyone has, at some time, been moved to curse at someone; certainly I have, and more than once. But, I would not want my work, or anyone else's, condemned for that reason.

While I was somewhat taken aback by Greenfield's paper, I was not at all prepared for the storm that greeted his presentation. I have often thought back on that afternoon and have tried to reconstruct what happened. It seems, in retrospect, that the Americans and Australians were most indignant. I think this was because he attacked the professors of educational administration where it hurt the most; he attacked the theory which formed the substance of what they taught. I suppose it was an attack on the priesthood, and the priests responded with a vengeance. On the other hand, the presentation was received with wild enthusiasm by most of the English present. I think this was because the paper dealt with phenomenology at such length and with unrestrained enthusiasm. It was almost as though Greenfield was a saviour leading the chosen people out of the wilderness of positivism. The two extreme feelings released simultaneously created a chaotic situation rarely seen in academic circles. The response to the paper was given by A. R. Crane who gave a reasoned response which seemed out of place at the time. He said that after Greenfield's paper we now had another way of doing research. Twelve years later it seems as though that was the best possible response.

The essence of the Greenfield critique may be found in two papers: one given at Bristol (Greenfield 1975), the other in the *International Encyclopedia of Education* (Greenfield 1984). The second paper reveals a sharpening of many points and an apparent major change in Greenfield's thinking. While the first paper was largely an exposition of a phenomenological approach to organisations, the second mentions the word phenomenology only once (Greenfield 1984). Further, Greenfield did not read his paper at Bristol but he spoke from it, and in his presentation he went

beyond the paper at many points. My discussion of his critique is, therefore, based on three resources: his two papers and my recollection of his speech.

Greenfield hit hard at the theorists (largely Halpin, Griffiths, Getzels, Simon, and March) because they split values off from facts, and it is true that we did. He contended that this resulted in descriptions of organisations that were lifeless, and that theory based only on facts devoid of values was of no use to administrators. This led, in part, to Greenfield's contention that the theory movement had produced little of consequence to practitioners. Greenfield was extremely vigorous, in his speech, in attacking quantification, computers and 'number crunching'. Again, this was, and is, a valid criticism, up to a point. Certainly there was, and still is, too much emphasis on quantification, and there are many problems which can be better studied using qualitative methods. On the other hand, there are problems best studied with quantitative methods.

Greenfield attacked the Getzels–Guba social system model on the ground that goals and roles were taken as given and were never questioned. He focused on system theory as an object of severe criticism on several scores: systems as 'natural' phenomena and, therefore, seen to be 'real'; social scientists anthropomorphising organisations through the system metaphor; systems imposing themselves on the people in them; and the notion that 'system' can be equated with 'organisation'. Greenfield was, and is, certainly correct when he says that the idea that 'an organisation is a system pervades education administration—and most other branches of organisation theory and administrative science as well' (Greenfield 1984). However, not everyone should be tarred with the same brush. In the paper of mine to which he refers, I said (writing in the third person), 'Griffiths uses system theory as a model to investigate the problem of change in organizations' (Griffiths 1964, p. 116). Nowhere in my chapter is there a contention that an organisation *is* a system. Greenfield is correct in criticising those who say that schools and other organisations are systems, since the term is no more than a metaphor, albeit a very important and useful one.

The major thrust of Greenfield's critique is, however, none of the above, but is, rather, epistemological. The first sentence of Greenfield's published version of his Bristol speech is the basic theme of his critique: 'In common parlance we speak of organizations as if they were real' (Greenfield 1975, p. 71). He then moves on to demonstrate that such is not the case and contends that organisations are 'invented social reality' (1975, p. 81). He thus argues that there are two schools of thought—one which views social reality as a natural system, and the other which regards it as a human invention—and that the two views have significant and far-reaching consequences for researchers and practitioners. He has sharpened his arguments in his latest paper and poses the question in this way:

> Theorists take one of two positions when they speak of organisations ... The first position sees organisations as independent of individuals. It sees them as real entities that are part of the natural world; it sees them as entities which individuals must adapt to or adjust to in some way. The second position sees organisations as *unnatural* but human statements that some people make to others to direct them how they should think of themselves and how they

> should relate to other people. The first position sees organisations as objective entities outside individuals; the second sees them as subjective understandings that people choose to live by—thereby making them real only through their own will and effort (Greenfield 1984).

Greenfield is, of course, in the second position, fully and unequivocally. This position has become more popular in the study of organisations as well as in other social sciences. It should be clear that practising researchers do not, and need not, accept either of Greenfield's positions. Frankly, they seem to ignore the question of what constitutes reality and go on with their work (see Griffiths 1983). In positioning the two poles, Greenfield attacks positivism with a vengeance, although it is interesting that at no time does he examine the tenets of positivism and show where they are erroneous. (This I have done in Griffiths 1982.) I think it is pertinent to offer an observation of Herda (1978) in her dissertation on the Griffiths–Greenfield debate. She commented that I was not a positivist and Greenfield was not a phenomenologist! I think she was right, and it was unfortunate that the discussion did not take place on a less rhetorical, more rational basis.

The Greenfield critique has been hailed in the British Commonwealth countries and largely ignored in the United States of America. As I point out in the readings included in this volume, ignoring the critique is unfortunate because what Greenfield did was to tell professors of educational administration that the social sciences are undergoing tremendous changes and that the philosophical and methodological bases on which the movement was founded (logical positivism) are now considered by most philosophers of science, and many social scientists, to be outmoded (see Griffiths 1982). As one reads the *Administrative Science Quarterly* (*ASQ*), especially the December 1979 and September 1983 issues, it is quite clear that a new approach to research and theory is in the process of emerging. It is difficult to say just what the approach is, but it is different. Perhaps the Summer 1983 issue of the *ASQ* is a good example of what I mean. The entire issue deals with culture as a metaphor. In educational administration it has been customary to say that theory must take culture into account, to consider it as a variable (the later versions of the Getzels–Guba theory, systems theory, and to some extent my decision-making theory did just that). However, the September 1983 issue of *ASQ* makes the plea that the culture *is* the organisation: no longer should researchers and theoreticians be content with the inclusion of culture as one of numerous variables. If one, then, is to study a school, one would have to describe the culture: the actions, thoughts, expectations and ideology of the participants. I might say that it is far more difficult to do this than it is to consider culture as a variable.

The newer approaches to administrative theory, then, turned things around. What was central, the formal organisation, becomes peripheral; what was peripheral, culture, now becomes central. One of the problems is, however, that there is no agreement on what should be central. Perrow (1978), for instance, raises the question of putting the notion that organisations serve many functions at the centre of a theory. What would such a theory look like? It would certainly be different, but it might run into great difficulty if it dealt with such concepts as power and authority, legal

control and decision making. But then conventional theory finds it impossible to explain the lack of women in management positions and the scarcity of minorities at the top of organisations, among other conundrums.

But to say that it is unfortunate that American scholars in educational administration have paid little attention to Greenfield (it is unfortunate that they have paid little attention to other critics as well), does not say that Greenfield is correct in all of his positions. In fact, I think he is wrong in a number of instances. However, Greenfield's great value is that he signalled a change in thinking in social science. This warning should not have been ignored. It should have sparked a detailed analysis of educational administration, but it did not. My own work since 1966, some of which is included in this volume, is one person's attempt to both initiate and respond to criticism of educational administration as an academic field of study.

The present

Several interesting directions are emerging in the study of organisations. Some of these are covered thoroughly in the readings included in this volume, so more space here is devoted to the trends that are more recent. I will first sketch the trends covered in the readings and then turn to the newer developments.

To my mind the most helpful book written in the past five years is that of Burrell & Morgan (1980). The book is a very careful analysis of sociological theory and contains an argument for carrying on research of many kinds. It is so useful because it, in essence, says: Stop arguing about what is *the* method, the *one* way to do research, and use all approaches, because each method allows the researcher to raise questions which could not be resolved by one approach alone. The authors argue that we are entering an era of paradigm diversity and, from a perusal of the *ASQ* and other journals, it appears that they are correct (see Griffiths 1969, 1979a, 1982).

Another trend clearly discernible is related to paradigm diversity but differs enough to cause it to be mentioned separately, and that is the use of new metaphors. We have restricted ourselves to very few metaphors: factory for school, system for almost everything. Morgan asks whether we would get a richer picture of organisations if we were to expand our range of metaphors. I think we would and present some ideas (see Griffiths 1982).

There is a growing belief that science is not so much the application of a particular method to the process of inquiry, but rather is concerned with the successful pursuit of knowledge (see Griffiths 1983). Closely tied to this view of science is the replacement of positivism and realism with a new philosophical approach called constructive empiricism (see Griffiths in press).

There is a general recognition held by most, but not all, theoreticians in organisational science, and by a very few in educational administration, that the world has changed a great deal and that our work should reflect those changes. These changes call for new types of administrators and new theories to guide them. The changed world scene means that the roles of people within organisations have changed. These views are explored in Griffiths (1977).

General organisational theory is the subject of severe criticism even though there is little in educational administration. 'Intellectual turmoil in educational administration' (Griffiths 1979b) is an effort at a comprehensive critique of both substance and method.

One of the clear present trends is the growing emphasis on qualitative research. The work of Egon Guba and Yvonne S. Lincoln in qualitative evaluation has been on the scene for a number of years and has gained a respectable following. Lincoln (1984) has indicated certain implications for educational researchers stemming from the introduction of qualitative methodologies: researchers are now beginning to realise that they must come to grips with epistemological questions; there are paradigm choices to be made; and epistemological choices lead to methodological choices. The turn toward qualitative research leaves unanswered, however, the problem of how knowledge claims are to be justified (Griffiths 1984). The positivists had a solution to the knowledge justification question which still works for certain mathematical and scientific situations, but simply does not work for anything else. The qualitative researchers have a long way to go on this question as do those following more traditional approaches. The demise of positivism made more visible the question of knowledge justification and little progress has been made in resolving this issue.

The emergence of qualitative approaches has brought with it a number of specific methodologies, probably the most popular being ethnography. With new methodologies comes the question of definition and I note with interest that Lutz (1982) made a valiant, but vain, effort to distinguish between ethnography and case studies. He contends that case studies are 'thin' descriptions while ethnography is 'thick' description, a distinction which seems to me to demonstrate how fruitless it is to attempt the task. Surely Iannaccone's 71 page case study of the Whitman School is a 'thick' description (Griffiths et al. 1962). So, too, were the case studies of Sargent & Belisle (1955), the descriptions of metropolitan school districts in McKelvey (n.d.), and so on. It may be that, although many contend that case studies and ethnography are different, there really is no difference. Their roots are much the same: they derive from anthropology. It seems to me that the important point is the renewed interest in the description of actual administrative and organisational situations. Notable among efforts now underway is Macpherson's doctoral study which attempts in-depth descriptions of a number of regional education officers. This type of study is badly needed in educational administration.

I note an interesting development in Greenfield's (1984) latest paper and in Peter Gronn's *Rethinking Educational Administration: T. B. Greenfield and his Critics* (1983) . Gronn, for instance, in discussing Greenfield's basic position as a subjectivist, makes references to the literature of perception. He uses, for example, a picture which is 'seen' in different ways by different viewers. In fact, it seems to me that the perception studies so popular in the 1940s and 1950s would be very helpful to both Gronn and Greenfield, since they offer experimental evidence on the very questions being raised.

Going back to the literature raises an interesting question: Is the fact that different people view the same stimulus in different ways explained better as a philosophical issue or, as has been done, as physical, social,

or psychological phenomena? The conclusion reached by the perceptual experimenters is that people do, indeed, view the world through their own somewhat distorting glasses. Many implications for administrative theory and practising administrators are to be found in the work of the perceptualists, and we should turn to them to rediscover their evidence.

Similarly, Greenfield is now becoming concerned with the concept of power, as well he should, since power is a key fact of organisational life. Without going into detail concerning the experimental evidence gathered about power in organisations, we can put together the concern with perception and the concern with power, and wonder if we are not headed in the direction of what was called the human relations movement (Griffiths 1956). The base for the movement was an impressive body of social science research which focused on the way people live in organisations. Much of what Greenfield and Gronn write about would seem to be dealt with better as an application of that kind of research rather than as phenomenology or some other variety of philosophy. It could be that a return to the old human relations movement might well be in the cards.

In this introduction to *Administrative Theory in Transition*, I have sketched my recollections of the theory movement, Greenfield's critique, and developments in organisation science and educational administration. In my papers, which date from 1966 to the present, I try to deal with the theory movement, Greenfield's critique, and other developments which affect us in educational administration.

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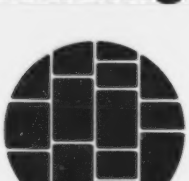
Readings



1 Theory in educational administration: 1966

Daniel E. Griffiths

There is little theory-building in America at present. Indeed, the only accurate way of describing the present-day scene is to say that theory-building in educational administration has "plateaued." Following a brief period in the '50's when several theories were presented to students of administration, there has been little production of original formulations. Some research has been generated by certain of the theories, and textbooks written in the '60's generally reported both the theories and the research. It must be said, however, that the promise of an era of productive theory-building has not materialized.

It is true that the titles of some books that have been published in the past year would seemingly indicate that theory production is proceeding at a record rate. Two books have been recently published with titles reminiscent of the theory-building movement, but their content does not extend our theoretical base.[1] A third is "old wine in a new bottle," interesting as a collector's item, but adding little to the modern scene.[2]

[1]Graff, Orin B., *et al. Philosophic Theory and Practice in Educational Administration*. Belmont, California: Wadsworth, 1966.

Saunders, Robert L., *et al. A Theory of Educational Leadership*. Columbus, Ohio. Merrill, 1966.

[2]Halpin, Andrew. *Theory and Research in Educational Administration*. New York: Macmillan, 1966.

It would be wrong to conclude, however, that the theory movement has been barren and unproductive. Present developments would hardly have been possible, if it had not been for the beginnings of theory construction ten years ago. The clue to what is now happening is to be found by re-reading Martin Trow's review of *Administrative Theory in Education*, a volume of papers edited by Andrew Halpin.[3] Trow stated in June of 1959, "What is conspicuously absent from most of these papers is any evidence of theoretically guided research in educational administration actually being done; substantive problems being defined and empirically studied; uniformities being discovered, related to one another, and accounted for; or any other form of theorizing actually going on in and through research."[4]

It may well have been that the appropriate reaction to the almost complete lack of theory claimed by Trow was to construct highly abstract and formally elaborate theories. It might be said that the tenor of the times was "theory for theory's sake," as a reaction against the non-theoretical anti-intellectualism of educational administration then prevalent. Perhaps the reaction was necessary in order to precipitate a change and to present an extreme position. The strategy (conscious or otherwise) worked, and the field did turn the corner.

The present thrust focuses on theorizing rather than on theory-building; on the formulations which should be constructed prior to theory-building rather than on the completed theory. There is now little interest in theories per se, but the interest is rather in answering fundamental questions using theories in the search for answers.

MODERN THEORIZING

Probably the most encouraging development is the quantity of empirical research now under way. Not only is there more research now than in the '50's, but the research is more varied. Detailed evidence on this point is difficult to obtain since there is little research on

[3]Halpin, Andrew (ed.). *Administrative Theory in Education*. Chicago: University of Chicago, Midwest Administration Center, 1958.

[4]Trow, Martin. Review in *Administrative Science Quarterly*, IV: 122–126, June, 1959.

researchers and their work. However, the number of projects in educational administration sponsored by the Cooperative Research Branch of the United States Office of Education has increased significantly since 1957.[5]

In addition, much significant research has proceeded under other auspices. The Carnegie Foundation's financing of work in the politics of education is one source of support that comes to mind immediately. While it must be said that the total amount of research is still disappointingly low, the changes in methodology and direction of the research including the better use of underlying theory, give promise of a fruitfulness that belies the still small number of projects.

This paper is too short to do more than contain examples of what the author believes to be the significant sectors of theorizing. Since the author is now much more concerned with action in administration than with thinking about administration, this paper may have a more provincial tinge than it would have had if written five years ago. What is significant in the theorizing movement? Studies are under way or have been completed in which a variety of administrative situations are described, appropriate concepts developed, relationships explored, explanations offered, and predictions ventured. Some studies are inductive in nature—that is, concepts and explanatory propositions are sought in the data. Other studies are deductive—that is, concepts and explanatory propositions are sought in models and descriptive data are used to test the propositions. Still other studies fit neither rubric since they deal with the application of models found useful in other fields such as economics or administrative decision-making. There are studies which employ theories to build taxonomies which in turn are tested against field data. And finally, there is the theoretical work done in a number of substantive disciplines.

Inductive Approach

The politics of education has proven to be a new and exciting area of research and theorizing in educational administration. It is a good topic to start with in this paper because the methodology used by

[5] Griffiths, Daniel E. "Research and Theory in Educational Administration," in Charters, W. W., Jr., *et al.* (eds). *Perspectives on Educational Administration and the Behavioral Sciences*. Eugene, Oregon: Center for the Advanced Study of Educational Administration, 1965.

researchers is the direct opposite of that advocated in *Administrative Theory in Education*. The researchers are either political scientists or close kin thereof. They approach their task with no formal theory (in fact the author is beginning to realize that political science is a field virtually devoid of formal theory). One can say that they work in a loose, easy, and relaxed fashion. It is with considerable delight that the author reports that they write interestingly and well. It is a pleasure to read their descriptions of political behavior. From these descriptions they develop concepts and relate these to one another in an attempt to understand what they have described. While they do not theorize as well as they write, one leaves their work with the feeling that at least it has been fun!

The example chosen is an unpublished paper by Iannaccone in which he pulls together the research he and his students have done on state government and education.[6] Iannaccone began his work with descriptions of eleven states made by teams from Syracuse University and Washington University.[7] He then developed a twelve-cell typology to categorize, in terms of structure, political behavior, and leadership groups, the ways in which professional educators went about the business of influencing the legislative process. The typology identified four phases in the development of influence, and the various states could then each be characterized as being in a particular phase.

From the model, Iannaccone and his students were able to raise such questions as: Does a particular state "fit" one of the phases? Do states move from one phase to another? If so, why? A number of studies based on the model and employing a variety of methodological approaches were undertaken in California. It was hypothesized that states do move from phase to phase, but in only *one* direction (to a higher numbered phase). Wiley tested this hypothesis by tracing the relationships between the California legislature and the education profession from 1849 to 1963.[8] He found that Cali-

[6]Iannaccone, Laurence. *State Government and Education*. Unpublished paper, New York University, 1966. See also Iannaccone, Laurence. "The Future of State Politics of Education," in Lutz, Frank and Joseph Azzarelli, *The Struggle for Power in Education*. New York: Library of Education, Center for Applied Research, 1966.

[7]Bailey, Stephen K., *et al. Schoolmen and Politics*. Syracuse University Press, 1962; Masters, Nicholas A. *State Politics and the Public Schools*. New York: Knopf, 1946.

[8]Wiley, W. Dean. "Political Interaction of Education and the California Legislature, 1849–63." Unpublished Ph.D. dissertation, Claremont Graduate School, 1966.

DEVELOPMENTAL CONSTRUCT OF THE STATE POLITICS OF EDUCATION

Characteristic	Phase One	Phase Two	Phase Three	Phase Four
1. Structure	Locally Based Disparate	State-Wide Monolithic	State-Wide Fragmented	State-Wide Syndical
2. Political Behavior	Entrepreneurial	Cooptational	Competitive	Coalitional
3. Leadership Groups	Squirarchy	Oligarchy	Polyarchy	Synarchy

Development over Time →

Source: Laurence Iannaccone, *State Government and Education*, *op. cit.*

fornia was in Phase 1 from 1849 until approximately 1919, Phase 2 until approximately 1962, and entered Phase 3 when the professional organizations were unable to elect the state commissioner. Wiley's study was historical in methodology and traced the movement of California from phase to phase. Another student, Bowles, using interviews and the participant observer technique documented the period of transition from Phase 2 to Phase 3.[9]

Working from descriptions of several states, Iannaccone and his students developed a typology, formulated hypotheses, tested them, and then restated these hypotheses. This is a good example of constructive theorizing in an inductive mode.

Deductive Approach

Iannaccone's approach was highly inductive, but the lesson is not that this is the heart of theorizing. Carlson's study of the adoption of educational innovations is largely deductive, but still very productive.[10] This study is being reported for two reasons: it demonstrates the deductive approach; and it exposes a basic weakness which under-

[9]Bowles, Dean. "Educational Pressure Groups and the Legislative Process in California, 1915-1966." Unpublished Ph.D. dissertation, Claremont Graduate School, 1966.

[10]Carlson, Richard O. *Adoption of Educational Innovations*. Eugene, Oregon: Center for the Advanced Study of Educational Administration, 1965.

lies much work in educational administration. The study will be discussed first and the underlying weakness second.

Carlson asked several questions concerning the adoption of educational innovations:

> 1. Why do some school superintendents adopt an innovation soon after its introduction whereas other schoolmen delay adoption until almost all others have accepted the practice?
> 2. Why are some educational innovations rapidly accepted and widely diffused while others just barely get off the ground?
> 3. What characteristics of educational innovations control their fate?[11]

Drawing from research on mass communication, Carlson constructed an explanatory framework for the varying rates of adoption of innovations. The scheme suggested that:

> the rate of acceptance of a new practice by individuals or adopting units is dependent on
> (1) the characteristics of the adopting unit
> (2) the way the adopting unit is joined to communication channels and sources of information, and
> (3) the position the adopting unit holds in the social structure of like units.[12]

Data to test this framework were collected in Pennsylvania and West Virginia. Since we are not as interested here in the results of studies as we are in their long range methodological significance it is only necessary to say that the framework was validated.

The weakness of much research into educational administration is that it is done by selecting a factor and then bending the evidence to prove that this one factor causes the outcome being studied. Virtually all of the research in the rate of adoption of educational innovations has been done by Paul Mort and his students; a total of some two hundred studies. In summarizing the results of this research Ross quotes Mort as saying, "If but one question can be asked, on the basis of the response to which a prediction of adaptability is to be made, the question is: How much is spent per pupil?"[13] Carlson, on the basis of his study, states, "In respect to the power of

[11] *Ibid.*, pp. 1, 2.
[12] *Ibid.*, p. 10.
[13] Mort, Paul R., as quoted in Ross, Donald H. (ed.). *Administration for Adaptability*. Revised edition. New York: Metropolitan School Study Council, 1958, p. xi.

the variable of expenditure per child to account for rate of adoption and *amount* of new educational practices, Ross's statement summarizing past research on adoption of educational innovations is not supported by these data."[14]

How could so much research go down the drain so easily? There are two possible explanations: one, an obsession with a single factor to which a man ties his reputation; and two, the ignoring of related research. Without belaboring the obvious, many studies are still being done today which are tied to a pet idea and which ignore the related research. Carlson's careful study is the antithesis of research tied to a single factor. In addition, his study grew out of an analysis of the related research. His work challenges a line of inquiry which has exerted an influence on American education far out of proportion to its worth.

Decision-Making Tools

The work of Iannaccone has been used to illustrate research that starts inductively; that of Carlson to illustrate research which starts deductively; and now the work of Hirsch will demonstrate relationships between theorizing and administrative decision-making.[15]

Hirsch's work, as discussed in this paper, relates to the use of two concepts: program budgeting and benefit-cost analysis. The two concepts are not unrelated, and their use enables the decision-maker to compare the implications of alternative programs. Neither concept is very complicated, but both are quite useful. Both concepts are relevant to macro- rather than micro-decisions, and are best employed in solving major problems involving governmental units rather than minor school district budgetary questions.

Program budgeting is the process by which the full cost of a program from the first year to the last is estimated. Hirsch has identified certain characteristics of a useful educational program that would allow for program budgeting:[16]

1. It should directly and effectively relate to the nation's major education objectives, and in this sense it should be end-product oriented.

[14] Carlson, *op. cit.*, p. 63.

[15] Hirsch, Werner Z. "Decision Tools for Education," in Charters, *op. cit.*, pp. 83–93.

[16] *Ibid.*, pp. 84–85.

2. It should lend itself to a meaningful breakdown into subprograms or program elements, which can readily be related to each other.

3. It should have administrative relevance and provide for administrative effectiveness.

4. It should directly relate to sources of funds and facilitate viable intergovernmental fiscal relations.

Benefit-cost analysis is an attempt to spell out, quantitatively, the direct and indirect costs and benefits of public education and to specify the persons or groups upon whom costs and benefits fall.

Hirsch has been successful in utilizing these two economic tools in his analysis of a number of problems such as the 1964 junior college proposal of the Educational Policies Commission and the financial status of the Clayton, Missouri public schools. There are however, many operational and conceptual difficulties associated with the use of program budgeting and benefit-cost analysis; measurement problems not being the least of these.

Taxonomic Research

Taxonomic research is concerned with determining the laws of classification. It raises such questions as: What are the characteristics by which units can be classified? Why should two kinds of behavior be classified in the same cell? What are the relations among cells of a system? As a study of the history of science demonstrates, taxonomic research has been a fruitful activity in many areas of knowledge.

One large scale taxonomic study which has been recently completed is reported in another volume in this series.[17] Very briefly the purposes of this study were to define more clearly the conceptual boundaries of organizational behavior in education and to give more precise definition to areas of knowledge which fall within these boundaries; to provide useful guides for selecting and abstracting content on educational administration which may be used for laying a sound base for informational retrieval systems; to furnish guides for use in developing instructional materials to prepare educational

[17]Griffiths, Daniel E. (ed.). *Developing Taxonomies of Organizational Behavior in Education Administration*. Chicago: Rand McNally, 1969. Contributing authors are Daniel E. Griffiths, Director; Frank Lutz, New York University; Stephen Hencley, Utah; George Chambers, Iowa; Morell Hill, New York University; Glenn Immegart, Rochester; Paul Cullinan, New York University; John Hemphill, San Francisco Regional Laboratory.

administrators; to encourage and provide guides for use in synthesizing knowledge on educational administration; to identify areas of educational administration where there is meager research and areas where additional inquiry is badly needed; and to develop and evaluate methods of taxonomic inquiry which would usefully facilitate further inquiry.

The study was divided into five steps:

1. The selection of theories for taxonomic development.
2. The collection of field data.
3. The development of classification schemata from theories.
4. The testing and refinement of the schemata.
5. Synthesis and evaluation of the schemata.

Selection of Theories

Four theories were selected within which development of classification schema was undertaken. These were: 1) General Systems, 2) Bureaucratic, 3) Compliance, and 4) Decision-Making.

The general systems approach was the most global of the four theories. Considerable difficulty was encountered by the researcher in this area due to the fact that, while those developing systems theory have written in overlapping and interlocking ways, one single general systems approach has not emerged.

The Weberian "ideal typical" specifications of a bureaucracy were selected as the bases for the schema developed within bureaucratic theory. With this was integrated the Parsonian concepts of "universalism" and "particularism" as used by Coladarci and Getzels in their work related to administrative theory. "Functionalism" and "disfunctionalism" as developed by Robert Merton completed the theoretical structure of the bureaucratic schema.

Etzioni's theory of compliance was selected as a theory that could account for the social process within the organization. Three types of goals, three types of power, three types of environment, and three types of tasks were identified by the theory. Eighty-one cells of expected behavior were generated from these twelve categories.

The final conceptual strategy to be employed was derived by Hemphill, Griffiths, Frederiksen, and others[18] in a very detailed

[18]Hemphill, John K., Daniel E. Griffiths, and Norman Frederiksen. *Administrative Performance and Personality*. New York: Bureau of Publications, Teachers College, Columbia University, 1962.

analysis of the behaviors of a large group of practicing elementary school principals. This yielded one hundred and forty-two categories which were used to categorize organizational behavior.

Data Collection

In order to determine whether or not the classifications generated by the four theories were empirically useful in classifying organizational behavior, it was necessary to provide specimens of organizational behavior which each taxonomist, using his own schema, would attempt to classify. Briefly, field studies were conducted in two educational settings. An eight month study took place in a public school district (K-12). Another study, two weeks in length, was conducted in a university in order to provide specimens which could, by the nature of the differences in the structure of the organizations, be expected to produce different kinds of organizational behaviors. Direct observation by field workers was combined with the use of 16 mm. sound film and tape recordings, producing a vast amount of data about the organizational behavior in the two organizations. From these data ninety specimens of behavior, called "Organizational Taxonomic Units," were developed. These are the specimens or examples of organizational behavior that were used to develop the taxonomies.

The Development of the Four Schemata

Each taxonomist independently began the development of classifications based on one of the four theories selected as having potential for the development of schemata. Thirty-six of the behavior specimens were made available to the taxonomists for this development. Upon the basis of this sample of specimens, in a type of "look-put" method, the schemata were modified and put into usable form.

Testing and Refinement of the Schemata

Next, the remaining fifty-four specimens were supplied to the taxonomists in order to discover if the schema were usable and to allow taxonomists to make final modifications. Then all ninety specimens were classified by groups of at least two individuals, each group using a different schema. Thus, all specimens were classified within each schema, and intra-scorer reliability was obtained.

Syntheses of the Taxonomies

Based on the generally held supposition that many theories in the social sciences tend to overlap and thus produce several concepts for the same behavior, the researchers expected to find frequent overlaps between their classifications.

Through both empirical inspection and computer analysis, the classifications of all schemata were analyzed. This was, however, of no avail, and the hope of emerging with a single taxonomy and moving toward a single theory was abandoned so far as this project was concerned. The project emerged with four taxonomies based on four theories. The conditions under which each of the four schemata should prove most useful to the analysis of educational organizations were specified. These specifications were theoretically based and should prove useful in the field of educational administration.

Multi-Disciplinary Research

Although much has been written on the value of interdisciplinary research in educational administration little has materialized. Projects such as TOBE have employed the talents of representatives of several disciplines working together, but there have been few other such efforts. There does, however, appear to be emerging a trend which the author terms *multi-disciplinary*, that is individuals representing various disciplines working individually on problems of educational administration. If the term interdisciplinary refers to a team of researchers from several disciplines, then multi-disciplinary should be construed to mean individuals working separately. There are many interesting efforts of the multi-disciplinary type that appear to be fruitful.

Historical studies by Callahan have produced insights into administrative behavior that could not have been arrived at by any other means. His most familiar work is *Education and the Cult of Efficiency* in which he advanced the "vulnerability" hypothesis to account for the behavior of school superintendents.[15] A later work, less well known, *Changing Conceptions of the Superintendency in Public Education, 1865–1961*, signals Callahan's expansion of interest

[15]Callahan, Raymond E. *Education and the Cult of Efficiency*. Chicago: University of Chicago Press, 1962.

in the study of the superintendent.[20] In the later volume he established descriptive historical categories through the use of primary source materials. The categories were:

1. The Superintendent as a Scholarly Educational Leader	1865–1910
2. The Superintendent as Business Manager	1910–1929
3. The Superintendent as Educational Statesman in a Democratic School	1929–1954
4. The Superintendent as an Educational Realist	1954–1965

The testing of hypotheses and the establishing of descriptive categories are two facets of historical research which Callahan has demonstrated to be very fruitful.

A descriptive analysis of style constitutes one of the most interesting new methodologies to be employed in educational administration. Wiley, a graduate student at Claremont at the time of writing, has done a fascinating little study comparing the literary styles of Paul Mort and Roald Campbell.[21] The study covers a modest sample of the writings of each part of *Principles of School Administration* and *Introduction to Educational Administration*, and is significant more for the methodology employed than for the outcomes.

Wiley's method consisted of a study of (1) imagery (metaphor, symbol, simile, personification); (2) vocabulary (jargon, cliché, etymological roots, frequency of key words); (3) the author's reading as indicated by documentation and both direct and indirect allusion; and (4) the general patterns of organization and emphasis.

Wiley concludes:

> Professor Mort represents in his life and work in the 1920's and 1930's the American tradition of empiricism, the tradition of dealing practically and effectively with real and current problems; and yet in his philosophy he views the meaning of his work in quite a different tradition, the transcendental.

[20]Callahan, Raymond E. *Changing Conceptions of the Superintendency of Public Instruction, 1865–1964*. Cambridge: NESDEC, 1964.

[21]Wiley, W. Dean. "Transcendentalism and Empiricism in the Intellectual History of Modern American Education: a Descriptive Analysis of the Style of Two Textbooks in Educational Administration." An unpublished paper, Claremont Graduate School, 1966.

> On the other hand, Professor Campbell, representing the intellectual outlook of the years after World War II to the present, may be seen as being completely in the empirical tradition, the tradition of the puritan and yankee trader, of Ben Franklin, of Mark Twain and the post-Civil War realists, of William James and Dewey (insofar as Dewey can be considered a realist).
>
> The two books chart a major turn in the intellectual history of educational administration, a shift from the idealistic emphasis on what should be to the social science insistence upon what is.

Callahan and Wiley present but two examples of the multi-disciplinary approach to the study of educational administration. Many more could, of course, be mentioned.

CONCLUSIONS

What conclusions can be drawn from this view of administrative theory in education in 1966?

1. The emphasis now is on theorizing, not on theory building. Studies are being done in which concepts are developed, related to each other, and tested against empirical data.

2. Theorizing is proceeding in a variety of ways—a highly desirable development. The study of administration should proceed through as many methodological approaches as possible.

3. There appears to be little theorizing concerning some of the major problems of educational administration: collective negotiation, integration, and more effective ways of organizing to provide education. The realm of public policy making has received little attention.

4. Significant bits of research, theory, and theorizing are not being "followed-up." Much research which opens an area gets no further because no one picks it up.

5. The search for one encompassing theory (if anyone is searching) should be abandoned. There is need for a number of theories concerning the various aspects of administration. Just as there is no one theory of anatomy, but theories of circulation, the nervous system, etc., similarly there should be theories of decision-making, personal relations, structure, etc., etc. Each theory may help in the understanding of the totality.

6. We have learned that a modest approach to theory pays off. Theorizing in terms of basic understanding of administration is proving more fruitful than did the highly abstract and formal theories of a decade ago. However, we would not be where we are without the battle of 1956.

D. E. Griffiths, 'Theory in educational administration: 1966', in G. Baron, D. H. Cooper & W. G. Walker (eds), Educational Administration: International Perspectives, Rand McNally, Chicago, 1969, pp. 154-67.

2 Another look at research on the behavior of administrators

Daniel E. Griffiths

When writing this chapter, I had an overwhelming feeling of déjà vu: I have certainly been here before. My previous papers on research in educational administration were attempts to view the field as a whole.[1] In general, I concluded that research in educational administration was not a robust child, and now, twelve years after my last effort, I can report that it is not a robust adolescent—a view shared by Immegart, who did so well in 1975 what I tried to do in 1959 and 1965.[2] The prognosis for a vigorous adulthood is dismal. The major reasons for this progression of sickly stages is that too few professors of educational administration have the interest or the competence to do research. As editor of the *Educational Administration Quarterly*, the only educational administration periodical in the United States that presumes to be scholarly, I am constantly appalled at the papers that we receive. Most of the writing can be typified as "using an atrocious style to spoil what might otherwise have been a mediocre idea."[3]

In contrast to previous papers in which I attempted to be complete and to buttress obvious points with appropriate statistics, I shall give a personal view of the difficulties I see in research in one aspect of educational administration, namely administrator behavior, and raise issues and suggest directions that interested researchers might explore. Further, I shall restrict myself to what I consider to be the three major matters of concern: the level at which research is done, the problem of transferability of research findings from one field of administration to another, and theory development.

Level of Research

C.P. Snow put our problem well when he wrote in his essay "Science and Government": "Most of the concepts that administrative theorists use are at best rationalizations, not guides to further thought; as a rule they are unrealistically remote from the workaday experience."[4] He might have added that the language we use in our research is equally remote.

I have been concerned about doing research and developing theory on the "real" behavior of "real" people for a long time, but only recently did I become aware that my colleagues and I use a rather abstract language to talk about

administrator behavior. This was first brought to my attention by Thomas B. Greenfield in Bristol, England, when he called for a phenomenological basis for research in administration. I have since read in phenomenology and have been influenced by David L. Denton, who insists that education, as "first-person experience," must be talked about in terms of the experiences of individual people, not in nomothetic terms. Instead, he notes, virtually all of the words employed in organizational theory refer to "definitions which are established by stipulation, by formal rule, or by ordinary usage."[5] He calls this vocabulary of abstract definitions "steno-language," and he says it cannot be used to talk about everyday experience. He then states a premise:

> Instead of starting with a standardized language, with its cannon of exactness, which precludes all but the most limited use of terms and sentences stating idiographic, novel, open, and multidimensional experience, let us start with that which is given to us in ordinary experience which is sharable, verifiable, mutually observable.[6]

Denton later states:

> Given the objective nature of steno-language, one cannot construct a theory of organization, or a theory of anything, which will be consistent with the presuppositions of steno-language and, at the same time, account for, open the door to, the idiographic, simultaneous, protean characteristics of individual persons in the operation.[7]

He suggests that organization—to be meaningful—must be grounded in the ordinary experience of individuals and talked about in appropriate terms:

> All of this would suggest that to talk of organization with the existential language of our ordinary experience would be to talk of autonomous groups, spontaneous order, workers' and students' and faculties' control, and the principle of federation.[8]

I am not so sure that one would end up with autonomous groups and the like, but I am quite certain that the use of nonabstract language to describe everyday experience would lead us in different directions in research and theory.

This discussion of the abstractness of our organizational language brings to mind a recollection of Heisenberg by one of his students, Felix Bloch. They were out walking and were talking about space. Bloch wrote:

> I had just read Weyl's book, *Space, Time and Matter*, and under its influence was proud to declare that space was simply the field of linear operations. "Nonsense," said Heisenberg, "space is blue and birds fly through it." . . . This may sound naive, but I knew him well enough by

> this time to fully understand the rebuke. What he meant was that it was dangerous for a physicist to describe Nature in terms of idealized abstractions too far removed from the evidence of actual observation. In fact, it was just by avoiding this danger in the previous description of atomic phenomena that he was able to arrive at his great creation of quantum mechanics.[9]

One of the achievements of the past twenty years is that we have developed a vocabulary that is now used by professor and practitioner alike to describe administrative situations. Halpin discusses this and laments the fact that the use of the vocabulary demonstrates an understanding that is only "word deep."[10] I am concerned that the use of this sophisticated language is deceptive. For example, I was discussing the creation of the 1972 Fiscal Review Group at New York University with a social scientist now turned management consultant. He accounted for the membership of this Task Force on the grounds that the members (all deans) were the "power elite" of the university selected because their positions in the "power structure" would ensure "compliance." (Note the vocabulary.) The real reasons were these: only the deans understood the budget, and only deans had no routine responsibilities and could be freed for six months to do the work. For a larger example of how sophisticated concepts lead one astray, read Baldridge's study of New York University.[11]

The first concern is the level at which research is done in educational administration; by this I mean two things. First, administrators are studied en masse by means of surveys.[12] Second, administrator behavior is studied through the use of such abstract concepts that the behavior is obscured.

Administrators as Individuals

While there is, no doubt, value in the kind of information that can be gleaned from questionnaires—average age, height, and weight, place in family, religion, birthplace, salary, and so forth—that which is really significant in understanding administrators is omitted. There should be a great deal of research on educational administrators as individuals: how they function in their jobs; how they learn the "territory"; what styles they develop; whether the styles are consistent throughout their careers or change as situations change; what makes them effective; indeed, what is effectiveness in educational administration? It is true that, for instance, some progress was made toward understanding administrator styles in the in-basket studies, but these have not been followed up.[13] Admittedly, the research involved a large number of principals, and we cannot be certain of the style of any one of them. The knowledge we need can be obtained only by the intensive study of individuals through observations, and descriptions should be written in terms of ordinary experiences expressed in ordinary language.

The book *Lyndon Johnson and the American Dream* by Doris Kearns is an example of what I am talking about. Professor Kearns describes in great detail the way LBJ behaved. Following is an abstract of a section discussing his early years:

In 1931 Johnson got a job as congressional secretary to a Texas representative. Moving from Texas to Washington he undertook to "learn" Washington just as he had learned Johnson City where he lived, San Marcos where he went to college, and Cotulla where he taught. He moved into the Dodge Hotel, which was occupied by seventy-five congressional secretaries. His floor had only one bathroom. The first night he took four showers so that he could meet and talk to as many people as possible. The first morning he brushed his teeth and washed his face five times—at ten-minute intervals—again to meet people. He chose five young men who he thought knew the most and made them his teachers. In the cafeteria he would push to the front of the line and gobble down his meal so he could ask questions while others ate. Note that he considered people the chief source of his Washington education, but he also read voraciously. He read *The New York Times, The Wall Street Journal,* the three daily Washington papers, and the Texas papers. Each evening he collected the daily *Congressional Record,* copies of pending bills, pamphlets, booklets, and committee reports and relaxed with them in bed the way others might relax with a good mystery. It was said of him, "This skinny boy was as green as anybody could be, but within a few months he knew how to operate in Washington better than some who had been here twenty years."[14]

The situation in which Johnson was placed determined whether he was successful. He could encompass all the San Marcos Teachers College but not the House of Representatives. The House was too big, with a highly structured, formal authority system. The Senate, on the other hand, fell easily to him. It was small. He got to know everyone. The power structure was sloppy, most of the senators did not care to dominate it, but Johnson did. He could personally confront and coerce every senator. He knew everything about them and used this knowledge to gain his own goals. As President he succeeded in his first two years because he put Kennedy's program through Congress—which he knew so well—but after that things fell apart. He could not learn all the aspects of the Presidency using techniques that were so successful in his early life, and apparently he could not learn new ways. Kearns concluded that the Presidency simply cannot be handled by a man with Johnson's characteristics.

The general approach used by Kearns is of value to us. She described Johnson's behavior using existential language and made attempts to explain the behavior largely by using Freudian concepts. My reaction is that the behavior of LBJ, a most complex human being, must have a better explanation than the fact that his mother disliked his father; however, the theoretical bases for explaining administrator behavior are weak, and it may well be that there is no better way. Nonetheless, the book is an example of one way in which the appropriate data can be collected.

Another source for understanding the people who become administrators is their autobiographies. It is true that there are not many of them, but the ones in existence are revealing. I have in mind Frank E. Spaulding's *School Superintendent in Action in Five Cities.*[15] This candid and completely revealing story tells one more than a dozen of the traditional modern texts tell.

What I am asking for is research on administrators as individuals using either direct observation such as the Kearns study did or systematic analysis of biographies and autobiographies.

Doing Administration

The English have many nice ways with the language. For instance, they say, "Margaret is at Cambridge *doing* mathematics." I believe that administration should be considered as something that people do. In this sense it is similar to other things that people do: medicine, dentistry, mathematics, teaching, and baseball. Since administration is, I believe, something people do, research must be done on the actual practice of administration.

By way of getting started let us review a few days in the life of a university dean.

9:15 a.m. *Chaired meeting of committee convened by the president to prepare a long-range policy for the university's participation in intercollegiate athletics. The committee was comprised of three professors chosen by the Faculty Council, two students chosen by the Student Council, and two deans, one chosen by the president and the other by the chairman. The agenda included: review of plans for new sports center; examination of intramural athletics; and review of statement on the value of intercollegiate athletics.*

10:30 a.m. *Deans Group. A weekly meeting of the five deans of the School of Education, Health, Nursing, and Arts Professions. The meeting lasted through a sandwich lunch. The agenda included: final recommendations on tenure and promotion; discussion of a number of problems relating to the transfer of a research and training center into the school; and report on expanding the school's activity in research in special education.*

2:00 p.m. *Meeting of the Organizational Committee of the Study of the Common Undergraduate Program. The Committee was chaired by the vice-president for academic affairs and was comprised of the deans of the five undergraduate colleges, the five chairpersons of curriculum committees, and two students. The meeting was concerned with final plans for getting the study under way.*

3:30 p.m. *Met with the Director of the Research and Training Center Program of the Rehabilitation Service Administration to discuss problems emanating from a site visit of the school's Deafness Research and Training Center.*

4:30 p.m. *Met with the board of trustees of the school's Para-Educator Center. The discussion was focused on raising money.*

5:30 p.m. *Dinner with the national advisory board of the school's Deafness Research and Training Center. Meeting of the board followed the dinner and lasted until 10:00 p.m.*

Items in the mail included a request from a student to have his grades raised so he could enter graduate school and a copy of a letter from an ophthalmologist to the president of the University demanding that the school be investigated because the dean did not pull a TV tape of a Sunrise Semester session which included an optometrist on a panel of four specialists in learning disabilities. Quite a different day was this:

10:00 a.m. to 12 noon *Chaired faculty meeting. The agenda included: report on the university study of undergraduate education; report of the Faculty Welfare Committee which dealt with faculty loads, retirement benefits, faculty performance ratings, and the IRS action on tuition remission; report from the University Senate dealing with a uniform time module for classes, affirmative action, and some activities of the university board of trustees; and report by the dean on retention and attrition in doctoral programs.*

1:00 p.m. *Met with the assistant dean for administration on a problem dealing with moving a research institute.*

2:00 p.m. *Participated in a doctoral oral as a reader.*

A more typical day was this:

8:30 a.m. *Met with president and central administration of the university to discuss off-campus programs on Long Island.*

9:30 a.m. to 1:30 p.m. *Attended meeting of members of the New York State education commissioner's Task Force on Teacher Education and Certification.*

2:00 p.m. to 4:00 p.m. *Met with school's Department of Music and Music Education to talk about themselves, plans for the future, problems (such as reducing doctoral attrition), and for a review of economic indicators and new programs.*

4:15 p.m. *Met with chairman of Faculty Welfare Committee to discuss problems in two programs.*

5:45 p.m. to 9:00 p.m. *Dinner and welcome of audience to the Peter Agnew Memorial Lecture.*

Several sets of research questions surface after such a recitation, the answers to which would help us to understand administrator behavior and serve as a basis for improved theory.

The first observation is that the dean was constantly involved with other people. Solitary activity was rare and took place before 10:00 a.m. or outside

the office. While with a group, the dean was often, but not always, the chairman. The group activity was sometimes policy-making, sometimes problem-solving, sometimes information-receiving, and the dean's work varied with the group. What kind of behavior on the part of the dean would have been most appropriate? Appropriate to what? What skills should he develop? What personality predispositions should be dampened or reinforced? How would the dean know when he was effective?

Every administrator is concerned about how he uses his time. While the days described above do not constitute a scientific sample, they are not atypical. Were these activities the best use of the dean's time? Could he have spent his time more effectively doing other things? What are the criteria for use of time? There are few guides to this set of concerns; there is little knowledge for administrators to use.

There are many questions concerning the authority of the administrator in education. The fact that he is constantly in interaction with people in the organization means that there is the need to talk things out, to negotiate, to bargain, and to do it on a face-to-face basis. What is known and what should be known of the authority position of the administrator in higher education? How does the authority of the university administrator differ from the authority of elementary or secondary school administrators?

I have one last set of questions. Even the brief overview of a few days in the life of a dean that we are considering raises inquiries concerning the physical health and stamina of administrators. How much of the success of an administrator is dependent on his health? Are there people who are less vigorous and are just as effective? Just what part does physical health play in the success of an administrator? Similar questions can be raised about mental health.

In this chapter I have expressed concern about the level of our knowledge about administrators as individuals. I have raised many old questions, most notably that of the criteria of effectiveness. I have stressed the need to study actual behavior by observation and have deplored the abstractness that is now the vogue.

Transferability of Research Findings

I have become increasingly concerned with the uncritical use of theories and research findings from sources other than educational administration. Even the most perfunctory examination of our literature demonstrates that borrowing is the practice rather than the exception. As Immegart has noted, "Educational administration professors simply appear to draw increasingly on the literature of other areas in a 'pure' or applied sense . . ."[16] Many textbook writers have shifted from drawing upon other education sources such as texts, reports, and association publications for content to drawing more broadly on the literature

from a variety of professional fields and disciplines.[17] Textbook writers have also moved from the almost exclusive use of research from education and psychology for findings to the use of research findings from other professional fields and disciplines in 1974.[18]

My first attempt to use research findings from another field should have been enough to cause me to question the practice, but it was not. In the process of doing my dissertation (on the leadership of school superintendents) I constructed what I called the Teacher Reaction Form.[19] Twenty of the twenty-two items came from Helen Jennings' study *Leadership and Isolation*.[20] You may remember she studied behavior characteristics of leaders using a population of 450 girls in the New York State Training Schools for Girls. Even her claim that "... the institution population does not deviate too radically from the general population"[21] did not deter me. It did not deter John Hemphill either, since he used the same study to develop the LBDQ (Leadership Behavior Description Questionnaire).[22] Well, the story of the Teacher Reaction Form was that, while it was reliable (.90), it had no validity when used with teachers to distinguish successful from unsuccessful superintendents of schools. Not only did the mean total scores not distinguish, but no item had a "*t*" in excess of 1.08. I finished this part of my dissertation by writing, "It may be concluded that such things as sense of humor, ability to make a speech, courteousness, and the like do not distinguish 'successful' from 'unsuccessful' superintendents."[23] I should have added that items that distinguish leaders from nonleaders of delinquent girls do not distinguish successful from unsuccessful school superintendents—for which they should be grateful.

The Teacher Reaction Form and the PBDQ (The LBDQ adapted for use with principals) are highly correlated, as one might expect. When they were used in the in-basket study, there was virtually no agreement between ratings on their form and superiors' ratings of principals.[24] That marked the last time I ever used the Teacher Reaction Form. I wish others could say the same for the LBDQ.

I believe I used the Teacher Reaction Form as long as I did because it was a short, easily administered, highly reliable form and because the belief that administration is administration regardless of the setting. I now question that belief. There are others who are also questioning.

John Millett published *The Academic Community: An Essay on Organization* in 1962 in which he developed this thesis:

> *I believe ideas drawn from business and public administration have only a very limited applicability to colleges and universities.* [Millett's italics.] More than this, the essential ideas about business and public administration, such as they are, may actually promote a widespread and unfortunate misunderstanding of the nature of the college and university in our society.[25]

While Millett's thesis received little support—in fact, the literature of administration developed in the opposite direction—the occasional studies done

in higher education as well as critiques tend to verify his point. Hill and French in a study, "Perceptions of the Power of Department Chairmen by Professors," concluded, "The findings reported here tend to confirm the impression of a number of students that colleges are unique kinds of organizations."[26]

In the only comprehensive bibliographical analysis of the administration of higher education that I could find, Olive offers the hypothesis that existing theories of organizational behavior and administration may not be applicable to present-day universities. In support of her hypothesis she says:

> . . . governmental agencies and business corporations have an internal organization which is unlike that of the university. Among the differences mentioned are these: the goals of the university are not specific and clearly defined; the product or service produced is not tangible; the customer (student) exerts limited influence; the employees (faculty) are dedicated to their specialized fields, not to the employing institutions; and the decision-making process is diffused in a way not typical of other forms of organization. Such differences suggest the inadequacy of present theories on problems of university administration and the incontestable and urgent need for new thinking and new approaches to research in this field.[27]

There are few empirical tests of the relevance of theoretical work in one field to another field. One such study, which did attempt to use a theoretical frame developed in industry and government to categorize New York City school personnel, found serious differences.[28] These were summarized as follows:

> A significant distinction between Presthus' and our studies, however, occurs in the fact that some two-thirds of all teachers were found to be either pupil-oriented or intellectuals. Presthus has no categories even remotely resembling these, leading us to question the wholesale and indiscriminate application of studies of business, industry, the military, and the federal government to education.[29]

However, Blau's study, *The Organization of Academic Work*, offers the opposite conclusion. He attempted to determine whether academic institutions and government bureaus and private firms were homologous, that is, exhibited empirical regularities in size, differentiation in structure, and administrative structure. He concluded, "The administrative structure of academic institutions exhibits a remarkable homology with that of other institutions."[30] It may be that if one compares universities and colleges with other organizations on purely structural grounds, without studying how the structures work, they will appear to be similar. Or, it may be the more abstract one's concepts, the more things appear to be the same. Further, as will be elaborated below, the use of a particular model, in this case bureaucracy, may well bias the results the researcher gets.

The only theory developed exclusively from studies of educational institutions known to me is the Cohen, March, and Olsen theory,[31] and it bears no resemblance to any of the theories now in use in educational administration. I would feel more comfortable with the Cohen, March, and Olsen formulation if I could see some clear relationship between their research and the resulting theory; nonetheless, the theory appears to explain certain aspects of university governance. Their theoretical framework can be summarized as follows: Whereas most organizations can be called "organized anarchies" some of the time, public, educational, and illegitimate ones consistently display these characteristics. They operate on the basis of inconsistent and ill-defined preferences–fuzzy goals, if you will; unclear technology, that is, their own processes are not understood by their members; and fluid participation, that is, the members change frequently and devote varying amounts of time and energy to decision-making and, further, the audiences and decision-makers change capriciously. Decision-making in such organizations is described by the authors as the "Garbage Can Model," for obvious reasons.

It would certainly seem that there is no clear evidence to support the contention that administrative theories can automatically be considered to be general or universal. Further, it would appear that the theorists and researchers in educational administration must give very serious thought to the idea that schools, colleges, and universities are institutions that vary in significant ways from IBM, G.E., and G.M.

Educational administration is not the only field in which people are concerned with transfer of knowledge. Lichtman and Hunt reviewed a large number of theories of organization and concluded that "the extreme variability found within and among organizations renders one-sided normative theories less useful in understanding organizational behavior than models that recognize situational contingencies."[32] Whether one can safely borrow research or theory done in a particular type of organization is dependent on whether there is a reasonable degree of isomorphism with the situation in which the theory or research is to be applied. This might be ascertained by applying a set of criteria developed by Katzell[33] and described briefly as follows:

1. Size. The number of interdependent organizational members is a crucial variable when attempting to transfer research findings from one kind of organization to another. A number of researchers have related employee absence and turnover, communication difficulties, job satisfaction, and morale to large size. Although there is a need for more research on problems caused by size, it is clear that what holds for large organizations very well may not hold for small organizations.

2. Interaction and interdependence. This parameter deals essentially with structuring the work environment. Katz and Kahn, for instance, argue that where task interdependence is high, where creative requirements are minimal, and where identification with organizational goals is not required, a hierarchical

authority structure calling for close supervision is indicated. Where the opposite circumstances obtain, a more democratic alternative should be used for maximum efficiency.[34]

3. Personalities of organizational members. The motivations, expectations, abilities, and other personal qualities seem to be of paramount interest in determining policies and practices.

4. Degree of congruence or disparity between organizational goals and the needs and goals of members. Organizations vary according to goal internalization of their members. The extremes are illustrated by workers on assembly lines and scientists in research companies or universities.

5. Who has ability and motivation. As Katzell says:

> . . . It would seem necessary to mold the organizational system so that it maximizes freedom of action and initiative for those who can and will take effective action, while eliciting compliance, support, or noninterference from others. Depending on the organizational loci of expertise and dedication, the appropriate work system may vary widely from situation to situation.[35]

I find Katzell's guidelines useful in helping to determine whether a piece of research done in industry, for instance, is applicable to an educational organization. Possibly, a set of rules might be devised to test the transferability of research findings. Another research task that needs doing is the actual testing of research findings together with the publication of that which is applicable to educational institutions. Even this would have to be interpreted with care, since there is such a wide variation among educational organizations.

Contemporary Theory Development and Educational Administration

In this section I shall argue that the paradigm presently used in educational administration is neither useful nor appropriate because it is no longer fruitful in generating powerful concepts and hypotheses; it does not allow us to describe either modern organizations or the people in them; and, as a result, it is not helpful to administrators.[36] I shall further contend that the fault is not so much in the methodology employed as in the substance. In other words, the tools and methods of research (commonly called the scientific method) are adequate, but what the tools and methods are used on is not.

The paradigm consists largely of the Getzels-Guba social systems model, role theory, decision theory, bureaucracy, and varieties of systems theory.[37] These theories hold many assumptions in common. They assume that organizations have goals that the members strive to attain; that there are roles, sets of expectations for the members which are agreed upon (the nomothetic dimen-

sion); that behavior is more or less governed by a set of rules (bureaucratic structure); that decision-making is a systematic process; that only legitimate power is employed; and that merit is superior to politics. Administration, organizations, and organizational behavior are viewed as essentially orderly and rational. Organizations function regardless of the individuals within them. These theories of administration are "great man" theories which assume that if the administrator were capable enough he would be able to comprehend and resolve all situations in a satisfactory manner. This is a view of organizations and administration familiar only to a committed researcher of the old school.

The argument is, then, that it is necessary to view organizations with different spectacles, that the theories we have been using have limited our view of what an organization is and how it operates.

There is, however, a basic question that must be discussed first. The question is "What is theory?" and, more particularly, "What is a theory of administration?" As I read various uses of the word *theory* today, I am more and more inclined toward, but have not yet accepted, Conant's position:

> The reader may be reminded that the words *theory* and *hypothesis* are frequently employed to describe both conceptual schemes and models, or pictures, explaining such schemes. A discussion of the definition of theory and hypothesis is often given in elementary texts, but I think such considerations of doubtful value. Because of the ambiguity it is well, perhaps, to avoid as far as possible both words in discussing the evolution of science.[38]

It is disturbing in reading Kuhn, considered by so many in educational administration to be the author of the definitive work in the history of science, to note the loose way in which he uses the words *theory* and *paradigm*. One diligent critic counted at least twenty-two different uses of *paradigm*.[39] Instead of being pleased at this recognition of his creativity, Kuhn discounted the different uses as "stylistic inconsistencies" and claimed to have used the term in only two ways.[40] For us, at this point, the following from Kuhn is of importance:

> Scientists themselves would say they share a theory or set of theories, and I shall be glad if the term can ultimately be recaptured for this use. As currently used in philosophy of science, however, theory connotes a structure far more limited in nature and scope than is required here. Until the term can be freed from its current implications, it will avoid confusion to adopt another. For present purposes I suggest "disciplinary matrix": "disciplinary" because it refers to the common possession of the practitioner of a particular discipline; "matrix" because it is composed of ordered elements of various sorts, each requiring further specification.[41]

I disagree. *Disciplinary matrix* is no improvement at all, and those who pervert the *term* theory will have a field day with *disciplinary matrix*. My view is very similar to that of Blalock:

> Ideally we might hope to achieve a completely deductive theoretical system in which there would be a minimal set of propositions taken as axioms, from which all other propositions could be deduced by purely mathematical or logical reasoning. (Note the similarity to Feigel.) More realistically we might take the model of the completely closed deductive system as an ideal which in practice can only be approximated.[42]

I believe we should attempt to approximate the use of the word *theory* to mean a set of assumptions from which propositions can be deduced by methematical or logical reasoning. I hope that we in educational administration reach consensus on this use and put a stop to the wide usage which has, for all practical purposes, rendered the term useless.

At this point I would remind everyone that there is much in administration that very probably lies outside the scope of theory. The practice of administration is largely an art and reflects the personal style of an administrator and the environment in which the person functions. Much of this lies beyond the reaches of theory as we know it.

Critique of the Current Paradigm

The paradigm currently used in educational administration has conceptual weaknesses, and it is not always useful for describing administrative situations.

Concepts: There is considerable evidence that the current paradigm is inadequate. Gross's work, for instance, calls into question the concept of role as it is used in educational administration.[43] The Aston studies indicate that Weber's concept of a single bureaucratic type is not a universal concept, since bureaucracy takes different forms in different settings. Riemann generally confirmed the findings of the Aston study,[44] but Mansfield contends that abandonment of the bureaucratic type is premature.[45] The evidence is strong enough, however, to call previous thinking about bureaucracy into question.

Systems theories and derivatives thereof also have shortcomings. Wildavsky, in a devastating article in *New York Affairs*, demonstrates convincingly that PERT (Program Evaluation and Review Technique), CPM (Critical Path Method), MBO (Management by Objectives), and PPBS (Program Planning and Budgeting System) rarely work. (In fact, he argues PPBS does not work anywhere in the world.)[46]

The major test of theories is the power of the concepts and hypotheses that

they generate. I reviewed volume 12 of the *Educational Administration Quarterly* to note the power of hypotheses and relationships among variables. I judged that eleven articles dealt with administrative behavior. In four of the studies all of the hypotheses were rejected or all tests were nonsignificant. In four of the studies no more than a third of the tests resulted in significant relationships. In three, the hypothesized relationships were confirmed. I concluded that the concepts and theories employed in these studies could hardly be called powerful. I suspect that what is published in the *Educational Administration Quarterly* is typical of our field. I recently sat in on a doctoral oral where the thesis that was defended was the sixth on conceptual systems theory. Only one of the six came up with significant findings.

I am not saying that one should not publish nonsignificant findings, but I presume that no one intentionally does studies that come out this way. The presumption is that the theories are powerful and, as such, suggest the possibility of producing significant findings.

Describing Administrative Situations: I shall present only two additional examples of the problems involved in using the current paradigm in describing administrative situations.

Of all the elements of society that have changed, that which has changed most significantly is the individual. Perhaps the case may be made by contrasting two statements, one by Simon in 1950 and the second by Lord Morris in 1974. In discussing the concept of authority Simon, writing in his classic *Administrative Behavior,* said, "The superior frames and transmits decisions with the expectation that they will be accepted by the subordinate. The subordinate expects such decisions, and his conduct is determined by them."[47] Lord Morris, in speaking of the same relationship said, "The peoples do not want to be governed, and clearly they do not believe there is any real and final necessity to be governed."[48] Both statements are a bit extreme; however, each is used to describe a central tendency. Clearly, the way in which people view themselves and the relationships to others in organizations has changed to the point that the theories of administrative and organizational behavior we have been using are no longer applicable. They do not describe organizational behavior, nor do they predict such behavior. They ignore the basic change in authority relationships, which has been under way since World War II and which accelerated in the early 1960s. This is probably the single most important change in organizations.

An interesting test by Bleecher of Barnard's criteria for judging the authoritativeness of organizational communications is also relevant to my point. He found that although teachers understood a set of accountability and assessment criteria issued by the Michigan State Education Department they felt them to be: (1) inconsistent with the purposes of the school organization as they understand them; (2) not compatible with their personal interest; and (3) not able to be complied with, mentally and physically.[49] This was in spite of a

lengthy and apparently logical attempt by the Department to set goals of the schools of the state. The theoretical framework of Barnard could not be used to describe what appeared to be a relevant organizational situation.

New Directions

I should like to sketch some of the new bases upon which emerging administrative theories must be constructed if they are to be used to describe administrative situations and to predict administrative behavior.

Garbage Can Theory. Clark and Guba in their study of schools, colleges, and departments of education find that decision-making fits the March, Cohen, and Olsen description.[50]

Environment-Situation. Taking von Bertalanffy's formulation, Emery and Trist developed a concept, "the causal texture of the environment," to account for those processes in the environment that are among the determining conditions of the exchanges between the organization and its environment.[54] Emery and Trist identify four types of causal textures following studies of change problems in hospitals, prisons, and educational and political organizations: placid, randomized environment; and turbulent fields. They also state that most of our troubles arise from the environment itself rather than from within the organization (a conclusion I am not entirely ready to accept). By a process that is not entirely clear, the authors feel that an analysis of the environment resulting in turbulent fields implies that McGregor's Theory *Y* is the proper course of action. This means that a new set of values must be developed–which they feel will take a generation.

Loose Coupling. Weick puts forth the concept of "loose coupling" as a way of describing organizations. He proposes that elements of an organization are often tied together loosely and that this incorporates a surprising number of disparate observations about organization, suggests novel functions, creates stubborn problems for methodologists, and generates intriguing questions for scholars.[52] I think the concept should be simply "coupling," since there are obvious examples of both tight and loose coupling. While Weick stresses loose couplings, I suggest that one reason why schools innovate so slowly is because they are so tightly coupled to their communities.

Informal Organization. Modern theories have attempted to deal with the attitudes and feelings of people in organizations through the concept of informal organization. The idea was that if the goals of people could not be met through the formal organizations, then the members would join together to seek their

goals outside the formal structure. Denton states, "Informal arrangements will develop for the simple reason that the life forces, the spontaneity, the elan, the ordinary experiences of persons overflow the bounds of any organizational chart."[53] Are informal structures central to organizations rather than peripheral as we have assumed? Remember that Chester Barnard developed the concept of informal organization, and he was the president of New Jersey Bell. How would he have viewed the informal organization if he had been a lineman?

Unions. Theories of administration and organization are written as though there were no unions or other formal employee groups existing within organizations. Theories of organization in education must be devised to incorporate the individual as a member of at least two formal groups. I suppose, after talking with my union friends, that there must also be an informal power structure within the union as well as within the educational institution. This means that each individual assumes at least four postures, formal and informal in each organization.

The formal decision-making structure of each school district or university must also be reconceptualized, because the union replaces building faculties in the public schools and senates in the universities as the decision-making body. Although there is no faculty union at New York University, for example, there is a plethora of formal groups all involved in varying ways in the formal decision process. Some of these are: Faculty Council, Deans Council, Student Senators Council, Central Administration Officers Group, President's Office, and the Management Council. There are two focal points at which they meet to develop policy: the Senate Budget Advisory Committee and the Senate. In addition, there are several unions of nonprofessional people whose policy issues are settled through collective bargaining. I have never seen a theoretical formulation that has attempted to incorporate these various elements in a university's decision-making process.

Idée Fixe. Philip Johnson was recently quoted as saying, "Let us celebrate the death of the *idée fixe*. There are no rules, only facts. There is no order only preferences."[54] For us, as administrators and professors of educational administration, it is particularly applicable because our theories have been based on the idée fixe.

The Individual. Although the phenomenologists make a strong case for treating people in theories as individuals without the use of steno-language or categorization, they offer no examples of what this means in practice.

It is true that present efforts at theorizing in educational administration employ concepts stated in sociological, psychological, economic, or anthropological language, and these are abstractions. For instance, we talk of a person occupying a role. Role is a sociological abstraction. The phenomenologist would

prefer to speak of the person as a basic fact. As Vandenberg puts it for education, "The task of educational theory is the restoration of the wholeness of educational phenomena as they appear within the educating perspective."[55] The ultimate task for educational administration is, I presume, very much the same.

If the person is considered the basic fact, then his values, his feelings, his attitude, what Denton calls his "soul," would be reproduced in some way. But would we then have a theory? Remember that theories are important only to the extent they give rise to the activities that Kuhn calls "normal science," that is, the testing of hypotheses derived from the theories. If the individual is the basic fact, how is he incorporated into theoretical statements unless steno-language and categorizations are used?

Rather than attempting to build theories in which individuals are central, we must try to construct theories that reflect the actual balance as it exists among people, organizations, and environment so as to be of value in specific situations. As noted, the theories presently employed in educational administration are essentially "great man" theories, which assume that if the administrator is capable enough he will be able to comprehend and resolve all situations in a satisfactory manner. It seems to me that a number of recent studies would lead to precisely the opposite conclusion, namely, there are situations which no administrator, however capable, can comprehend and resolve unless changes are brought about to alter the environment or the organization. I have in mind the Nicholas et al. study of the climates of four elementary schools and the differences in administrative behavior found in each.[56] In addition, there are now several studies that attempt to build classification systems of climates and environments.

An experimental study by Frederiksen et al. employed simulation to test the effect of climate on the people working in it.[57] The major finding was that the mean productivity score, that is, the amount of work completed, is significantly affected by the consistency of climate conditions. More important are the four climates that were contrived and the fact that it was clearly and significantly demonstrated that the climates made a difference in administrative performance.

Clark and Guba conclude in their study of schools, colleges, and departments of education that, "The culture of institutions of higher education is predominately idiographic (that is, emphasizing the self-actualization of the professor) rather than nomothetic (that is, emphasizing the goals of the institution)."[58] Administration in such an organization must surely demand a theory different from that which is appropriate for IBM or the Army, which are far more nomothetic than the university.

If theory is built on observations of organizations--one by one--it seems apparent that individuals (except for the President or certain mavericks) form groups to attain their personal goals and, often, the organizational goals. Thus, public school teachers have developed strong unions and associations designed to

achieve the goals of individuals. Even universities in which individuals have traditionally worked on their own are moving toward unionization. The individual as a member of a group of his own choosing is a fact of organizational life.

Development of the New Paradigm

What, then, are some of the dominant ideas which might well be influential in the development of the new paradigm?

1. The end of the idée fixe in modern life. Just as in modern art the artist pictures life in nonrepresentational ways, so too will the social theorist recognize that his work should produce theories that encompass the essence of modern life.

2. Theories will have to develop new approaches to the concept of authority and acknowledge the fact that people do not want to be governed.

3. Because of the change in authority relationships, the key administrative process in organization is very likely to be bargaining and not necessarily collective bargaining. It could very well be that the Barnard-Simon theory, which is essentially a bargaining theory, will be prominent in the new paradigm.

4. New theories are likely to be highly specific and focused on particular types of organization. The search for a general theory of administration will be long delayed.

5. There will be numerous efforts to build theories using ideology and values for axioms, with propositions predicting behavior dependent on the axioms. It is doubtful that these efforts will succeed.

6. Emerging theories will likely use situations and situational variables as axioms.

7. Methodologically, emerging theories will be far more complex than present theories.

8. New concepts entirely different from ones with which we are familiar will be used (for example, coupling).

Summary

I have attempted to point to three directions in which research is needed. These include researching what it is that administrators do; using nonabstract language; and establishing criteria of effectiveness for administrators. I also raised the question of the transferability of research findings, and suggested some guidelines. Finally, I asked for new approaches to developing theories of educational administration.

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3 Theories: Past, present and future

Daniel E. Griffiths

The history of intellectual style is replete with periods that are distinctive because the scientific view of the world changed. We have had, for instance, the Copernican Revolution, which moved man from the centre of the universe to the periphery, and the Einsteinian Revolution in which even time was shown to be relative. We are now on the verge of another scientific revolution; this one, like the others, is attracting little attention, not only from the public, but even from the producing researchers.

I think the issue has been put best by Suppe in *The Structure of Scientific Theories* (1977), which is an outgrowth of a symposium held in 1969 at the University of Illinois. He says:

> During the last five or six decades there have been important shifts in philosophical thinking about scientific discovery and the growth of scientific knowledge. The positivists distinguished the context of discovery and the context of justification, dismissing the former as the subject matter of history or psychology. The only aspects of the growth of scientific knowledge relevant to philosophy were the inductive justification or confirmation of knowledge claims and the incorporation of older theories into more comprehensive theories via intertheoretic reduction. The resulting view of scientific knowledge was a static one which, ignoring the dynamics of scientific progress and being tied to an untenable observational/theoretical distinction and associated epistemology, led to a highly distorted portrait of science and the knowledge it provided, which had little to do with the epistemic activities science actually was engaged in. Rejecting such a view, a group of 'young Turks'—including Hanson, Feyerabend, and Kuhn—started examining scientific practice and the history of science and developed *Weltanschauungen* views that, unfortunately, made scientific knowledge a social phenomenon in which science became a subjective and, to varying degrees, an irrational enterprise.
>
> More recently, philosophers such as Lakatos, Toulmin, and Shapere have attempted to steer a middle course between these two extremes wherein science is a rational enterprise concerned with obtaining objective knowledge of the real world (Suppe 1977, pp. 704–5).

We are, in our research methodology in educational administration, in the period in which we are acknowledging the weakness of positivism but have not yet entered the period of post-positivism. We appear to be rejecting the approach that gave the world its greatest scientific productivity and are now seeing a whole raft of approaches such as phenomenology, ethnomethodology, symbolic interaction, neo-symbolic interaction, and the sociology of the absurd. Probably the only assumption shared in common by these approaches is '. . . that each sees human beings as creating, or constructing, their own social reality in interaction with others' (Morris 1975, p. 168). We are also paying attention to the obvious shortcomings of traditional organisational

theory, a topic I shall touch upon later. Then, too, there is a variety of Marxist approaches, which appeal to many social scientists.

In this paper I shall deal with the topic largely from the orientation of the philosophy of science. The topic could be handled chronologically as has Scott (1981), or in terms of substantive criticisms as I did (Griffiths 1979). The role of a philosopher of science is to analyse theories and their role in the scientific enterprise. (For a more elaborate statement see Suppe 1977, pp. 3–5) So, I will discuss the nature of theory-building in the past, what it appears to be now, and what it might be like in the future.

The past: Logical positivism

There is little question that theory and research in educational administration has been, and still is, significantly influenced by the analysis of theory made by logical positivists. Culbertson's article (1983) is a critical analysis of theory in educational administration as it was affected by logical positivism.

While I shall not duplicate Culbertson's article—but I do recommend it to you—I remind you that the logical positivists did accomplish a great deal during the time of their ascendancy. What the positivists tried to do was to say how science was done, how new scientific knowledge came to be accepted, *and* to say that this was the *only* way. The English philosopher, Ayer, put the strengths of logical positivism thus:

> It brought about a great emphasis on clarity, and a great opposition to what might be called woolliness. It issued an injunction to look at the facts, to see them as they are, to get rid of humbug. It was like the Hans [Christian] Andersen child . . . saying 'The Emperor has no clothes—there he is, parading around, but the fellow's naked' (Ayer 1978, pp. 130–1).

When asked what, in retrospect, he considered to be the main defects of logical positivism, Ayer replied, 'Well, I suppose the most important of the defects was that nearly all of it was false.' But 'what survives is the general rightness of the approach' (Ayer 1978, p. 131).

As a young man I was attracted to logical positivism because my training was in mathematics and science. I taught physical science from a historical case approach at Colgate, was thoroughly disgusted with what passed for research and serious thought in educational administration, and was influenced by a number of social scientists trained in the logical positivist mode (Halpin, Hemphill, Getzels and Gross). It seemed to me that the logical positivist approach was the proper antidote for the self-serving testimonials, the pseudo-theories of Mort and Sears, and the plain nonsense that constituted the field of educational administration. In retrospect, it was not a bad move. I have no intention of eulogising logical positivism, but I shall close these introductory remarks with a quotation from Scriven:

> Positivism brought . . . hardheadedness to philosophy and perhaps to some parts of science, at a time when it was needed. Hard heads usually have to be thick heads, and it is no surprise to discover in the cool of later years that the issues were not quite so simple as they then appeared. Nevertheless, revolutions are fought by men who lack finesse, and without them we would still be in a rather primitive state. We must pay tribute to the revolutionary while avoiding the mistake of deifying his doctrine (Scriven 1969, p. 209).

The first thing wrong with logical positivism in educational administration was that developments in theory started and stopped with it. While at the time it was introduced it was a strong purgative, the field soon became constipated. Science is a constantly changing, constantly emerging field. Unfortunately, there are too many people in edu-

cational administration who want to learn a theory and hold on to it for the rest of their lives. They want to *believe* in a theory. People who want to *believe* in a theory should be in a school of divinity, not a school of education.

The nature of science was well expressed in *Men of Ideas* (Magee 1978), a set of dialogues with outstanding philosophers:

> *Magee*—So we now have to think of science as a set of theories which are constantly being replaced by better theories—better in the sense of more accurate, or in the sense of richer, i.e. explanatory of more. Even the most successful and sophisticated theories we have, like those of Einstein and his most gifted successors, will be replaced in the course of time by theories yet unconceived of, from the brains of scientists yet unconceived.
>
> *Putnam*—That's exactly right. In fact, scientists themselves make this prediction: the main theories of the twentieth century—relativity and quantum mechanics—will both give way to some other theory which will supersede both of them. And so on, for ever (Putnam 1978, pp. 229–30).

If this is the case with physical science, how much more is it the case with the social sciences!

What precisely was and is wrong with logical positivism? I cannot, in this paper, review all of the literature devoted to dissecting the corpse, but I shall discuss some of the most significant objections.

Let us start with the central concept of logical positivism, *the 'received view' on the role of theories* defined by Putnam (1962, p. 240) as follows: '. . . theories are to be thought of as "partially interpreted calculi" in which only the "observation terms" are "directly interpreted" (the theoretical terms being only "partially interpreted", or, as some people even say, "partially understood")'. I refer you to Suppe (1977, pp. 16–23) for a detailed analysis of the 'received view'. For the purposes of this paper a more general discussion will suffice. The positivists saw theory-building and verification as a process in which propositions were deduced from theories and these propositions were tested by an observational statement in which the terms were non-theoretical and objective. The theory was then altered to account for the results of observations. Positivists used three vocabularies in their theoretical work: a logical vocabulary which included logical constants and mathematical terms, an observational vocabulary, and a theoretical vocabulary. As Suppe described the early version of the 'received view', the entire process was as follows:

(i) The theory is formulated in a first-order mathematical logic with equality, L.
(ii) The nonlogical terms or constants of L are divided into three disjoint classes called *vocabularies*:
  (a) The *logical vocabulary* consisting of logical constants (including mathematical terms).
  (b) The *observation vocabulary*, V_O, containing observation terms.
  (c) The *theoretical vocabulary*, V_T, containing theoretical terms.
(iii) The terms in V_O are interpreted as referring to directly observable physical objects or directly observable attributes of physical objects.
(iv) There is a set of theoretical postulates T whose only nonlogical terms are from V_T.
(v) The terms in V_T are given an *explicit definition* in terms of V_O by *correspondence rules* C—that is, for every term 'F' in V_T, there must be given a definition for it of the following form:

$$(x)\ (Fx \equiv Ox),$$

where '*Ox*' is an expression of *L* containing symbols only from V_O and possibly the logical vocabulary (Suppe 1977, pp. 16–17).

These vocabularies governed the entire 'scientific method' as seen by the positivists. This restricted version of science gave rise to some serious problems, most notably the theoretical–observational dichotomy.

The basis for the division appears to be as follows: The observation terms apply to what may be called publicly observable things and signify observable qualities of these things, while the theoretical terms correspond to the remaining unobservable qualities and things. At the simplest level, the theoretical–observation distinction was developed to interpret or even to verify theories. It was contended that a theory could be tested by deducing a theoretical statement from the original theory and testing it by an observation statement in which the terms were non-theoretical and objective. (Graduate students in educational administration around the world are addicted to this type of research.) Unfortunately, only in certain types of predictive studies does the distinction hold water. If, for instance, one is studying the effects of nutrition on the height of young children, one might have a theoretical statement concerning the nutrition of young children, and height could be observed with a metre stick. The observation statement in this case would be non-theoretical and objective. However, the measurements could not be used to interpret or explain the theoretical statement.

It is left to Putnam, however, to completely demolish the observational–theoretical dichotomy:

(A) If an 'observation term' is one that cannot apply to an unobservable, then there are no observation terms.
(B) Many terms that refer primarily to what Carnap would class as 'unobservables' are not theoretical terms; and at least some theoretical terms refer primarily to observables.
(C) Observational reports can and frequently do contain theoretical terms.
(D) A scientific theory, properly so-called, may refer only to observables. (Darwin's theory of evolution, as originally put forward, is one example.) (Putnam 1962, p. 241).

The theoretical–observational dichotomy is crucial to the two most frequently used theories in educational administration. The Getzels–Guba model, which has spawned the most research, is vulnerable since the observational statements are measured by personality inventories or other psychological instruments, the theoretical bases of which are ignored. Likewise, the use of Halpin's LBDQ to test hypotheses suffers from the same difficulty. Both types of research use observational measures which are theoretically based, subjective, and, in the case of Halpin, full of value judgments. The way the hypothetical–deductive method was and is used in social science and educational administration is not what the positivists advocated. Whereas they said that scientific knowledge accumulated through the testing of theoretical statements by observation statements that were objective, present practice in educational administration is to test one theoretical statement with another theoretical statement. This later practice does not necessarily lead to scientific knowledge.

The positivists' contention that the 'context of discovery' was of no scientific relevance and was of interest only to historians is a matter of serious concern to organisational theorists. A basic tenet of positivism was that the setting of a scientific discovery did not affect the discovery (one wonders how they handled the Heisenburg Effect). Organisational theorists today are convinced that the setting might be

the most important element in a social experiment or situation; certainly it is of such significance that it must be considered. Recent analyses of the Hawthorne Effect indicate that the behaviour of the women in the Relay Assembly Test Room can be explained more parsimoniously and more accurately by remembering that the experiment took place during the beginning of the Great Depression. Employees throughout the Western Electric plant in Cicero, Illinois, were being laid off and the women were well aware of this. It is more likely that they produced more, regardless of the kind of stimulus they received, thinking that higher production would lead to a retention of their jobs (see Rice 1982, pp. 70–4). When the Harvard team of social scientists ignored the context of their experiment they foisted an erroneous concept on the world that is still considered valid by many.

Others of the central tenets of logical positivism have met similar ends. Operationalism, for instance, was one of the concepts stressed in the 1950s in educational administration. Operationalism demanded that concepts be defined in terms of sets of the operations. Scriven reminds us, though, that there are grave problems with the concept of operation. As he says: '. . . in particular, grave problems about (1) whether an operational definition [of an operation] can be given, (2) what is to count as *one* operation, (3) what is to count as *an* operation' (Scriven 1969, p. 198). I am not in total agreement with condemning operationalism, having had some success in developing units in our study *Developing Taxonomies of Organizational Behavior in Education* (Griffiths 1969). We did define *one* operation to our satisfaction.

Scriven (1969, p. 199) makes the interesting point that, while operationalism was developed in physics, it found more use in psychology. I hate to think what psychology would be like today without the strictures of operationalism. Remember Wittgenstein's characterisation of psychology as having 'experimental methods and conceptual confusion'. The concepts would be a lot fuzzier without operationalism.

Much the same is true of the concept of value-free social science. Maybe it is a pipe dream, but when I see what has happened when the concept of 'value free' has been dropped, I would rather we, at least, kept it in mind.

Suppe summarises the era of positivism as follows:

> To conclude, virtually all of the positivistic program for philosophy of science has been repudiated by contemporary philosophy of science. The Received View has been rejected, as have its treatments of explanation and reduction. Its developments in inductive logic and confirmation theory continue to be influential, but the focus of such work has shifted from a positivistic-like concern with global induction to the development of probabilistic treatments of local induction. Also, the importance of induction and confirmation is coming to be sharply downgraded in contemporary philosophical thinking about the scientific enterprise and the knowledge it provides. Positivism today truly belongs to the history of the philosophy of science, and its influence is that of a movement historically important in shaping the landscape of a much-changed contemporary philosophy of science (Suppe 1977, p. 632).

So logical positivism has passed away. Where are we now?

The present

The present period in philosophy of science has been termed chaotic by Suppe. Many of the ideas and concepts that seemed so certain are no longer held. Take the word 'theory', for instance. Shapere (1979, p. 125) sums up the present by saying: 'There is today no completely—one is almost tempted to say remotely—satisfactory analysis of the notion of a scientific theory'.

When Putnam was asked about science he gave some interesting answers.

Magee – . . . But now that our whole conception of science has altered, it is not so much that some different method has come to be regarded as 'the' method, but rather that there is no longer thought of as being only one single valid approach to all scientific problems.

Putnam – There is a sort of paradigm of 'the scientific method' (a paradigm which itself is pretty vague, as I just remarked) which one occasionally finds pretty well exemplified, especially in physics. But even in physics there is a great deal of knowledge which doesn't, and shouldn't, fit the paradigm. I don't believe that there is *really* an agreement in our culture as to what is a 'science' and what isn't. Any university catalogue claims that there are subjects called 'Social Sciences', and that Sociology and Economics are sciences. But I would bet that if we asked anyone in the Physics Department whether Sociology is a science he would say 'No'.

Magee – Yes, but what grounds would he have for saying no?

Putnam – I think the real reason is not that the sociologists don't use the inductive method – they probably use it more conscientiously, poor things, than the physicists do. I think it's because they're not as successful.

Magee – So you think 'science' is now simply a term for the successful pursuit of knowledge?

Putnam – That's right (Putnam 1978, pp. 233–4).

The chaotic situation in organisational studies has come about because of the inadequacies of present theories to account for what is observed in organisations. In my paper 'Intellectual turmoil in educational administration' (Griffiths 1979), I attempted to catalogue and analyse the criticisms of organisational theory. The criticisms range from the fact that organisational theories ignore the presence of unions, and fail to account for the scarcity of women and minorities in top administrative positions, to the failure to account for the degree of external control that is exercised over practically all organisations, and that organisational theories strive to be universal but end up with only limited applicability. Then there is Perrow's criticism, now accepted by many, which questions the long-held assumption that organisations '. . . are, or can be, rational instruments of announced goals' (Perrow 1978, p. 247). The rejection of this assumption is central to what Perrow terms 'demystifying organisations'. The rise of ideologically based theories such as Marxism, the introduction of new research methodologies such as ethnographic studies, and the popularity of phenomenology all add up to a confusing picture. Snow (1971, p. 56) has said: 'The scientific process has two motives: one is to understand the natural world, the other is to control it'. This statement is as true for the organisational theorist as it is for the physical scientist. At present, we can neither describe our organisations nor control them, and this is the basis for the present situation.

There have been at least three efforts to order the present situation. Hage's *Theories of Organizations: Form, Process, and Transformation* (1980) is an elaborate framework through which he attempts a great synthesis. Benson evaluates the effort in this way:

> The difficulty is that the current state of the field does not make it susceptible to a great synthesis. Because the differences in the field are rooted in opposing methodological stances and paradigms, Hage's prodigious effort is unlikely to do very much to reconcile them (Benson 1982, p. 166).

Scott (1981), in a much more modest effort, analysed developments in organisational theory in terms of closed and open systems, and rational and natural models. He depicts the developments in Table 1.

Table 1 Dominant theoretical models and representative theorists for four time periods

Closed system models		**Open system models**	
1900–1930	1930–1960	1960–1970	1970–
Rational models	**Natural models**	**Rational models**	**Natural models**
Taylor (1911)	Barnard (1938)	Woodward (1965)	Hickson et al (1971)
Weber (1947)	Roethlisberger & Dickson (1939)	Lawrence & Lorsch (1967)	March & Olsen (1976)
Fayol (1949)	Mayo (1945)	Thompson (1967)	Meyer & Rowan (1977)
	Dalton (1959)	Perrow (1967)	Pfeffer & Salancik (1978)
	McGregor (1960)	Pugh et al (1968, 1969)	
		Blau & Schoenherr (1971)	

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This analysis is very helpful, but is not inclusive of the wide array of present activities. The analysis by Burrell & Morgan is more inclusive and is the one I shall use in this paper.

Burrell & Morgan (1979, p.xii) base their study on the view that 'all theories of organisation are based upon a philosophy of science and a theory of society'. The authors directed themselves from the beginning of their work to clarifying the confusion which now exists in social theory, including organisational theory. In their words:

> Our proposition is that social theory can usefully be conceived in terms of four key paradigms based upon the nature of social science and the nature of society. The four paradigms are founded upon mutually exclusive views of the social world . . . With regard to the study of organisations, for example, each paradigm generates theories and perspectives which are in fundamental opposition to those generated in other paradigms (Burrell & Morgan 1979, p.x).

The first strand Burrell & Morgan called either 'subject–objective' or 'German idealism–social positivism'. The authors raised four basic questions: ontological, epistemic, human nature, and methodological. The answer to each question lies on a continuum. With regard to the ontological question there are those who believe each person creates his or her own world while at the other end of the continuum are those realists who believe in an objective world, the same for all, a 'social world [that exists] independently of an individual's appreciation of it' (Burrell & Morgan 1979, p. 4).

When one considers epistemology one find the positivist who seeks regularities and casual relations as contrasted with the antipositivists who believe that social science is essentially subjective and that there are no laws or underlying regularities in the social world.

In the matter of human nature the continuum ranges from those who believe in free will to those who believe that people are controlled by their environment.

The methodological continuum ranges from the ideographic view of social science (only first-hand knowledge counts), as contrasted with doing research through the use of systematic protocols and other techniques.

The second strand is the regulation or order view of society versus the radical change view. Burrell & Morgan contrast the two views in Table 2.

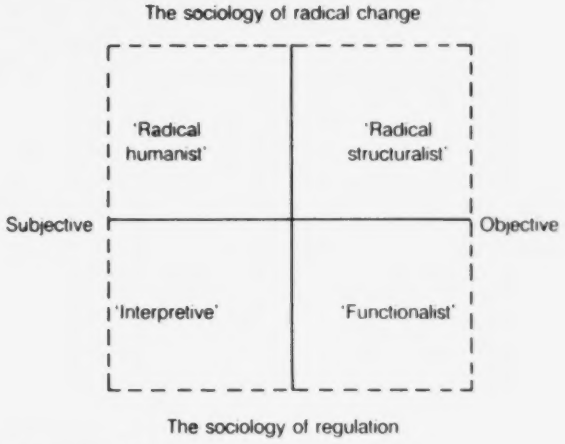
Table 2 The regulation–radical change dimension

The sociology of *regulation* is concerned with:	**The sociology of *radical change* is concerned with:**
(a) The status quo	(a) Radical change
(b) Social order	(b) Structural conflict
(c) Consensus (voluntary & spontaneous)	(c) Modes of domination
(d) Social integration & cohesion	(d) Contradiction
(e) Solidarity	(e) Emancipation
(f) Need satisfaction	(f) Deprivation
(g) Actuality	(g) Potentiality

Source: Burrell & Morgan 1979, p. 18.

The two dimensions form four paradigms: radical humanist, radical structuralist, functionalist, and interpretive (see Figure 1). Virtually all the theory we teach and use in educational administration is in the cell called 'functionalist organisational theory' (the lower right-hand cell). There are three sources of theories in this cell: practising administrators as, for example, Barnard, sociology of organisations such as Weber, and behaviour in organisations as exemplified by Simon. The characteristics of the theory are: regulation, objectivity, status quo, consensus, social integration, solidarity, need satisfaction, and actuality. Theories tend to be realist, positivist, determinist, and nomothetic. Some examples of theories that are functionalist and are used in educational administration are: Getzels–Guba social systems, Griffiths decision-making, Immegart systems theory, Barnard–Simon equilibrium theory, and contingency theory.

Figure 1 Four paradigms for the analysis of social theory



Source: Burrell & Morgan 1979, p. 22.

The interpretive paradigm is based on the belief that people build the social world in which they live. It is in the tradition of German idealism and has its roots in the work of Immanuel Kant. As Burrell & Morgan point out:

> Such a view does not allow for the existence of 'organisations' in any hard and concrete sense. Whilst certain schools of thought accept the concept of organisation and its use as an 'accounting practice' by which people attempt to make sense of their world, they do not recognise organisations as such. From the standpoint of the interpretive paradigm, organisations simply do not exist (Burrell & Morgan 1979, p. 260).

Studies of organisations within the interpretive paradigm are relatively rare and Burrell & Morgan discuss only efforts utilising ethnomethodological approaches and phenomenological interaction. They distinguish between the two as follows:

> Whereas the ethnomethodologist usually focuses upon the way in which individual actors account for and make sense of their world, the phenomenological symbolic interactionist focuses upon social contexts in which interacting individuals employ a variety of practices to create and sustain particular definitions of the world. They demonstrate how 'reality' and 'facts' are essentially social creations, negotiated through the interaction of various competing themes and definitions of reality (Burrell & Morgan 1979, p. 271).

Gephart's study (1978) and much of the December 1979 issue of the *Administrative Science Quarterly*, which is devoted to qualitative method, are examples of the ethnomethodological approach. The paper by Pettigrew (1979) is the only example of a study done in educational settings. The *Educational Administration Quarterly* apparently has yet to carry an ethnomethodology study. I was unable to find a study in educational administration done in the phenomenological symbolic interaction mode.

The anti-organisation theory paradigm is based upon radical humanism with roots in the French existential movement (Sartre) and German idealism (Kant, Fichte, Hegel). Burrell & Morgan state:

> The interpretive and radical humanist paradigms are both founded upon the notion that the individual creates the world in which he lives. But, whereas the interpretive theorists are content to understand the nature of this process, the radical humanists subject it to critique, focusing upon what they regard as the essentially alienated state of man (Burrell & Morgan 1979, p. 279).

Burrell & Morgan observed that, as of 1979 no one appeared to be studying organisations in the radical humanist mode, but if anyone does, the result will be '*anti-organisation theory*'. They conclude:

> Since the radical humanist perspective stands in fundamental opposition to that of the functionalist paradigm, reflecting a complete inversion of assumptions about the nature of science and society, anti-organisation theory will stand in fundamental opposition to contemporary organisation theory (Burrell & Morgan 1979, p. 310).

The radical structuralist paradigm has provided the framework for a vigorous critique of functional structuralism and therefore most of present-day organisational theory. While there are two strands to radical organisational theory, Marxist and Weberian, the Marxist approach is far more popular. The Marxist critique includes a number of variations called neo-Marxism, ethno-Marxism, and the Marxist dialectic. The Marxist view interprets organisation life in terms of power, conflict, contradictions, crises, dialectics, and class, and this type of interpretation is the basic thrust of the Marxist critique of traditional organisational theory.

Clegg (1977, p. 22), for instance, contends that 'Much of . . . organization theory has been developed at the interface of capitalist theory and capitalist practice in the academic institutions of business'. Heydebrand (1977, p. 85) argues that 'organizational theory, like other theories in the social sciences, has been dominated by powerful ideological forces which, taken together, have more or less successfully reproduced and legitimized the structure of capitalist society'. In contrast, Perrow (1978, p. 247) observes that 'the ethno-Marxist approach challenges conventional organisation theory on almost every score'. However, the Marxian dialectical approach is presented most clearly by Benson (1977), who sees the organisation as a concrete, multileveled phenomenon, beset by contradictions that continually undermine its existing features. He contends that 'there are four principles of dialectical analysis—social construction/production, totality, contradiction, praxis' (Benson 1977, p. 3). The central idea of social construction/production is that people are continually constructing the social world, which is Greenfield's (1975) major thesis. The concept of totality means that any particular structure must always be seen as part of a larger, concrete whole, not as an isolated, abstract phenomenon.

Contradiction is seen by some writers, such as Heydebrand, as the central principle of the Marxist critique. What it means is that the process of social construction contains contradictions. Heydebrand illustrates the concept of contradiction with a study of the organisational contradictions within judicial systems. In an effort to introduce efficiency and to increase productivity, both administrative and judiciary reforms have been introduced. This has meant, on the one hand, the introduction of administrators into the courts and, on the other hand, more judges and court personnel. These two developments tend to contradict each other with the result that the 'adjudication and the adversary process are increasingly replaced by negotiation, arbitration, and ultimately by administration' (Heydebrand 1977, p. 99).

The fourth principle is praxis, meaning action or activity or practice. Benson (1977, p. 5) defines praxis as 'the free and creative reconstruction of social arrangements on the basis of a reasoned analysis of both the limits and the potentials of present social forms'. What this means is that theories, for instance, reflect the social context in which they are created and the practical concerns of their creators. Theories also are deeply involved in the creation of organisations, and they guide actors in their organisational behaviour. From this tautology Benson (1977, p. 16) concludes: 'There is, then, a dialectical relation between organizational arrangements and organizational theories'.

There are obvious problems within the Marxist critique. It is, for example, full of inconsistency. The principle, 'social construction/production', that people create their own world, is contradicted by the concept of totality, which indicates *how* people should construct their world. Then there is Heydebrand's contention that ethno-Marxist theory is applicable to all organisations. This seems to be an example of the over-enthusiasm that characterises the traditional theorists, and Heydebrand is probably in error too.

Ethno-Marxism is interesting because it raises questions concerning traditional theory which must be rethought. The Marxist finds in every aspect of organisation a meaning significantly different from that seen by the organisational theorist working in the traditional modes. For the Marxist, rules are not so much ways of achieving the stated goals of an organisation as means of controlling people and exercising power; the hierachy represents not so much levels of ability as the maintenance of class structure. These are but two examples of how the schools of thought differ. It will be very interesting to see in what form organisational theory will emerge from the Marxist challenge.

The Weberian strand of radical organisation theory differs from the Marxian strand

in that its devotees draw heavily upon conceptualisations from political science. Burrell & Morgan depict the differences in Table 3.

Table 3 Some differences in emphasis between Marxian structuralist and radical Weberian approaches to radical organisation theory

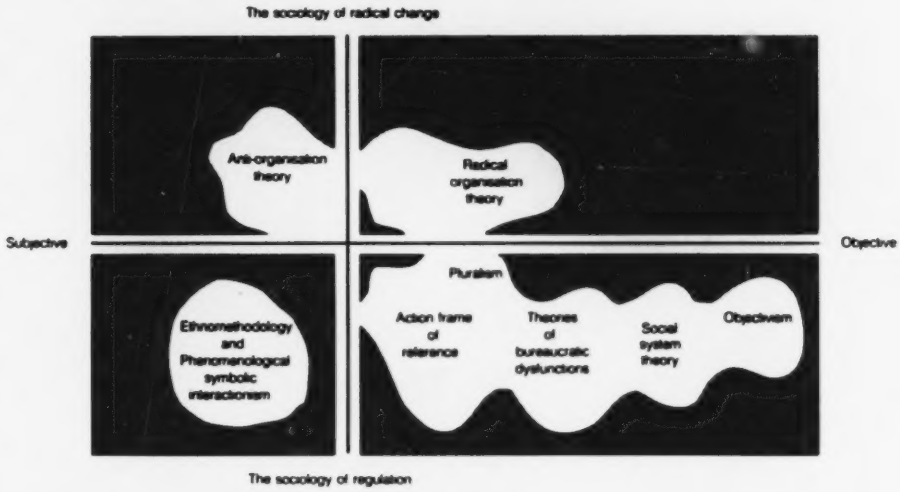
The Marxian structuralist approaches to a radical organisation theory *tend* to stress:	The radical Weberian approaches to a radical organisation theory *tend* to stress:
1 Political economy	1 Political science
2 Economic structures	2 Political administrative structures
3 Monopoly capitalism	3 Corporatism
4 Contradiction	4 Power
5 The catastrophe analogy	5 The factional analogy

Source: Burrell & Morgan 1979, p. 385.

The two strands are quite distinct, but at present the Marxian approach is getting a great deal more attention than the Weberian.

Figure 2 depicts the four paradigms discussed above.

Figure 2 The main schools of organisational analysis



Source: Burrell & Morgan 1979, p. 30.

In summary, the present era is characterised by an increasing volume of research by functional structuralists while at the same time the rate of criticism of the functional structuralists by scholars of the interpretive radical humanist, and especially the radical structuralists is also increasing. What at first glance appeared to be chaos now appears to be quite understandable. Morgan (1981) sees the present as a time of 'paradigm diversity' and indeed it is. I will now speculate on where this will lead.

The future

I am not, in this final section, attempting to predict the future or to act like a futurologist. I shall project some directions that are now in evidence and try to discuss how

these directions might develop. This section will also be shorter than the others, not because the future will be shorter than the past or present, but because, obviously, so little is known.

As we move into the future, I propose that we pay more attention than in the past to advice on the nature of science given to us by Snow:

> Scientists know their kinds of beauty when they see them. They are suspicious, and scientific history shows they have always been right to have been so, when a subject is in an 'ugly' state. For example, most physicists feel in their bones that the present bizarre assembly of nuclear particles, as grotesque as a stamp collection, can't possibly be, in the long run, the last word (Snow 1971, p. 190).

In this same sense, administrative theory is 'ugly'. The wildly diverse methods being advocated, the lack of power in the variables we use, the insignificance of our findings all lead us to the conclusion that where we are cannot be the final stage.

First, I think it quite clear that the era of paradigm diversity as depicted by Burrell & Morgan is here to stay for quite some time. Several kinds of activities will take place during this period. The criticism of the work of the functional structuralists will mount and there will be heated refutations. Research and theorising in the functionalist mode will continue, but, it will soon become clear that this work is changing. The first changes will be substantive: new theories will give a much larger place to the environment of organisations, to networks of related organisations, to the goals of others than executives, to unions, and, in higher education, to professors as managers. The theories will be closely related to the sociocultural context so as to be able to explain the place of women and minorities in organisations. In other words, the functionalists will respond first to the criticisms of the inadequacies in their theories and will produce work that is more reality-related, is more useful to practitioners, and will accumulate to more fruitful theory.

We can locate these methodological innovations within each of the four paradigms (Morgan 1981, p. 23). Each paradigm is based upon different assumptions and research methodologies will reflect these different assumptions. As Morgan (1981, p. 23) states: 'Such an approach offers the possibility of replacing debates about the merits of competing methodologies with a consideration of the merits of rival logics of research'.

Another outcome of paradigm diversity will be the emergence of new questions to be researched. For example, radical humanism advocates an ideology which places people first (Morgan 1981, p. 17). Supposing that in studying an organisation the researcher looked first at the people, not organisatonal goals and structures. What would the resulting theory look like? Looking at organisations through the assumptions of four different paradigms will produce far more, and different, questions than we have yet dealt with.

Although there is little evidence in the literature of educational administration to support the contention that non-functionalist research and theorising will increase, I believe that it will. Most, if not all, of the non-functionalist literature is criticism of the functionalist; this will either have to give way to significant research and theory-building, or cease to exist. Criticism in educational administration, carping, constructive or otherwise, cannot exist in isolation: it must be accompanied by research and theory that builds to substantive bodies of knowledge. With this as a basis, I believe that shortly there will be some research in the Marxist mode, and somewhat more in the ethnographic approach. Both hold promise for interesting new insights into organisations.

The era of paradigm diversity is already upon us and will be more in evidence in the near future. It will bring improvements in present functional structurist theories, innovations in research methodology and new questions to research.

Second, there is another development already on the scene, which many believe will be a dominant philosophical trend of the future. As Suppe (1977, p. 709) states: 'The semantic conception of theories . . . is the only serious contender to emerge as a replacement for the Received View analysis of theories'. I have discussed the origins of semantic analysis, which is usually credited to Wittgenstein, in an earlier paper and will not repeat it here (Griffiths 1979, pp. 56–8). More recently others have written on the same subject. Bates (1982), for instance, discussed the metaphor and its crucial importance in thinking about administration. He points out: 'Shifts in the fundamental metaphors which we use to explore and interpret the world of nature and the nature of society have far reaching repercussions' (Bates 1982, p. 7). He illustrates the significance of a change in metaphor by recalling '. . . the shift from an animistic view of the universe to a mechanistic one brought about by Newton and his philosophical colleagues, Bacon and Locke, involved a major shift in attitude towards nature which became for the first time viewed as accessible, knowable and *controllable*' (Bates 1982, p. 7). In American education the metaphor of a factory has dominated thinking about how schools are set up and operated. I have often wondered what schools would be like if the metaphor of a hospital had been used instead.

A more comprehensive discussion of metaphor is found in Morgan's (1980) article. He contrasts the two major metaphors used in organisation theory as follows:

> Whereas in the machine metaphor the concept of organization is as a closed and somewhat static structure, in the organismic metaphor the concept of organization is as a living entity in constant flux and change, interacting with its environment in an attempt to satisfy its needs (Morgan 1980, p. 614).

It is certainly clear that the metaphor one chooses dictates the way one thinks about and conceptualises organisations. Morgan urges us to expand the metaphors we use:

> Viewing organisations systematically as cybernetic systems, loosely coupled systems, ecological systems, theatres, cultures, political systems, language games, texts, accomplishments, enactments, psychic prisons, instruments of domination, schismatic systems, catastrophes, etc., it is possible to add rich and creative dimensions to organisation theory (Morgan 1980, p. 615).

The future, then, should certainly see a significant increase in semantic analysis as a basis for theory development. This, together with paradigm diversity, offers some interesting possibilities. These two approaches hold out hope that the future will produce organisaitonal theory that is, in Snow's terms, less 'ugly', more a thing of beauty.

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Evolution in research and theory: A study of prominent researchers

4

Daniel E. Griffiths

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The mode of scientific inquiry that was dominant in the 1950s and early 1960s is described, and the work of four prominent sets of researchers of that period is analyzed in an effort to determine the degree to which the research that was conducted actually followed the tenets of the prevailing philosophy. Then, the much more complicated conditions of the present are examined in the light of the multiple, divergent, and conflicting ideologies that are now being espoused. Four currently prominent sets of researchers are selected, and both the theoretical and methodological characteristics of their efforts are assessed. The general conclusion of the analysis is that the researchers of the present are ignoring the criticisms of functionalism as well as the new approaches being advocated, and they are still working loosely within the framework of logical positivism.

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There is no shortage of articles devoted to the enumeration of research studies and the appraisal of their quality in educational administration. The latest of these appeared in the summer of 1982,[1] and little would be gained by a similar endeavor so soon thereafter. A somewhat different task is, therefore, undertaken in this study. In reviewing research studies done in the late 1950s and subsequently to the present time, is it possible

to discern an evolutionary trend? If so, how would its description read? Likewise, is there an evolutionary trend in administrative theory over the same period of time; if so, what would its description be? Further, is there a relationship between the two?

This article will not deal with all of the research done in educational administration; those studies that focus on school finance, school buildings, transportation, pupil personnel services, and the like will be omitted. Only those concerned with the administrative process, including administrator behavior, were considered, and of these, only a few selected: the work of four sets of prominent researchers of the late 1950s and early 1960s, and four sets of prominent researchers in the 1970s and early 1980s. The selection of those designated as prominent is admittedly arbitrary; another writer might have chosen others. In addition, the theoretical frameworks of the past and present will be analyzed and related to the research studies under investigation.

THE PAST

There is little question that the mode of scientific inquiry that dominated the period that coincided with the beginning years of the University Council for Educational Administration (UCEA) was logical positivism. The influence of this mode of scientific thinking came suddenly and dramatically to a field best characterized by intellectual provincialism and naiveté. Without tracing all of the events that led to the introduction of logical positivism to education administration, the formal debut was made at the 1954 meeting of the National Conference of Professors of Educational Administration (NCPEA) at Denver. Several university centers of the Cooperative Program in Educational Administration (CPEA) financed the attendance of six social scientists at the NCPEA. They made presentations to the Theory Interest Group chaired by Daniel E. Griffiths. The major impact was made by Jacob Getzels, University of Chicago, Arthur Coladarci, Stanford University, and Andrew Halpin, The Ohio State University. They not only carried the message that the social sciences had a great deal to offer professors of educational administration, but also that social science was to be conveyed in the form of theory and that theory was in the logical positivist mode. Denver was followed by the Coladarci and Getzels monograph on theory in educational administration in 1955,[2] by the first UCEA seminar at the University of Chicago in 1957 (appropriately entitled "Administrative Theory"), and also in 1957 by the NCPEA-

sponsored book (prompted by the Denver conference) under the title of *Administrative Behavior in Education*.[3] Halpin edited the report of the Chicago seminar, which was entitled *Administrative Theory in Education*, and it appeared in 1958.[4] Griffiths's *Administrative Theory* was published in 1959, and the National Society for the Study of Education issued *Behavioral Science and Educational Administration* in 1964.[5] One decade saw theoretical production of the quantity and quality not seen before or since, and this literature was dominated by logical positivism. The lone voice against positivism was that of Orin Graff, who favored a far less scientific approach—one that would have gained more support in 1983 than it did in 1964.[6]

What was the logical positivism that was advocated and accepted? First, it was contained in the definition of theory as presented by Halpin at Denver and attributed to Fiegl: "I propose to define a 'theory' as a set of assumptions from which can be derived by purely logico-mathematical procedures a larger set of empirical laws."[7] Theories were to be verified by deriving a theoretical statement (a statement composed of theoretical terms) from the theory, and testing it with an observational statement in which the terms were nontheoretical and objective. This is the theoretical-observational dichotomy.

Another tenet of positivism that was enthusiastically embraced was that concepts must be defined in terms of sets of operations. Operationalism was seen as an antidote for the plethora of fuzzy concepts in the literature of educational administration. Then, there was also great concern over the "is-ought" dichotomy: "is" being facts and "ought" being values. It was not so much that theoreticians were to deal with facts and not values as it was that they should distinguish carefully between them. Finally, there was the underlying assumption that organizations were like physical science entities: They were rational, subject to universal generalizations, and the administration of all organizations had more in common than not.

This then was the scientific ideology of the times. Theoreticians accepted the approaches and tenets of logical positivism in their discussions of theory. What, in fact, did they accept in their own research?

Major Research Studies

The late 1950s and early 1960s saw several major research studies. Although these could not be called typical, they were, rather, exemplary;

they set the pace for research that was to follow. The studies selected for discussion here are those by Getzels and Guba; Gross et al.; Halpin; and Hemphill, Griffiths, and Frederiksen.[8]

Getzels-Guba. The social systems model developed by Getzels and Guba had a greater impact on theory and research in educational administration than any other single model. While there are later papers on the subject that reflect changes in the conceptualization, "Administration as a Social Process" was chosen for use here because it is the cleanest, clearest discussion of the rationale underlying the model.

Without question, Getzels and Guba were very heavily influenced by the logical positivist movement. In the paper referred to above, Getzels described the model, derived hypotheses, and tested them. In identifying their intentions, Getzels stated the following:

> *The model was constructed with three specific criteria in mind: (1) the model must provide a set of integrated concepts and relations capable not only of answering questions already asked in administration, but of posing questions that still need to be asked; (2) the concepts and relations must be operational in that they not only give direction to our understanding, but simultaneously provide blueprints for investigation; (3) the model must be able to handle as many of the commonplace or familiar issues in administration as possible within a single set of concepts and relations. In short, we sought a model that was at once heuristic, operational, and that had the elegance and power of parsimony.*[9]

The method of science employed was to state the assumptions upon which the theory was based, derive hypotheses, and test them through the use of the theoretical-observational dichotomy. The concepts were operationalized. The hypotheses were not deduced in a logico-mathematical process as called for by Fiegl, but rather, they were arrived at by reason and occasionally, it seemed, by intuition. This, in fact, set the mode of operation in educational administration. Hypotheses were arrived at by a process of reasonable deduction and intuition; they looked as though they should follow from the theory.

It should be noted that the instruments of observation employed to verify the theoretical hypotheses were not objective or nontheoretical; they were generally psychological tests filled with theoretical concepts. At the time, this was not seen as the major flaw in the positivists' approach, as is now the case.[10] It should be noted further that the Getzels-Guba model was, in its early years, a closed system model. Later, more dimensions were added that took into account the influence of the environment.

Although the Getzels-Guba model clearly reflected the influence of logical positivism, it deviated in the way in which hypotheses were derived and verified.

Gross. The first empirical test of role theory was *Explorations in Role Analysis: Studies of the School Superintendency.* This study was little noticed by professors, researchers, and practitioners in educational administration. This was unfortunate because Gross and his associates concluded that the role concept "has yielded few significant hypotheses of theoretical importance."[11]

The methodology of the study was in the logical positivist mode. A Fiegl-like definition of theory was used in that a set of assumptions was adopted and hypotheses deduced. As with Getzels and Guba, it cannot be said that the hypotheses were deduced by logico-mathematical means but, rather, through a process of reasonable deduction. The theoretical statements were tested by the results of interviews, psychological instruments, and self-reported self-perceptions—hardly the objective observational data called for by positivists.

While Gross and his associates made a valiant effort to operationalize their concepts, they freely admitted defeat.[12] It could be that role concepts were of such a nature as to make operationalization impossible, and this appears to be the conclusion of the researchers.

Although *Explorations in Role Analysis* was clearly in the logical positivist mode, it also deviated in the way in which hypotheses were deduced and verified. Since the researchers attempted to ascertain the influence of community variables on the role of the superintendent and school board members, it can be said that they employed an open-systems model.

Halpin. What can be said of the research methodology of Andrew Halpin, the most prominent disciple of logical positivism? Halpin's research was confined largely to the development of a paradigm for research on administrator behavior and two instruments: the Leader Behavior Description Questionnaire (LBDQ) and the Organizational Climate Description Questionnaire (OCDQ).

The first striking conclusion that results from a study of Halpin's work is the almost complete lack of theory in any of it. The LBDQ and OCDQ have no theoretical bases at all. They come as close to the naked empiricism that Halpin derided as is possible.[13] The two questionnaires resulted from massive factor analytic studies, not from a process of hypothesis deduction and testing. This is not to say that factor analysis is not a useful research approach, but rather, it is not what Halpin advocated for others.

Halpin described his paradigm as follows: "However, as it now stands, the paradigm is *not* a theory."[14] He saw it instead as a way of generating a theory, or theories, of administration.

Halpin did, however, show allegiance to positivism when it came to definitions: They had to be operational, and in virtually all cases, he was successful in making them operational. The most notable exception was the concept of authenticity. Although admitting that "authenticity is a tricky concept; it does not lend itself readily to operational definition,"[15] it would have been closer to fact to say authenticity did not lend itself to operational definition, *period*. But he seemed obsessed by the concept, regardless of the lack of success in defining it.

Halpin's research reflected little of the influence of logical positivism except for his use of operational definitions. His paradigm included the influence of community factors and of the *Zeitgeist* on the behavior of the administrator; nevertheless, both the LBDQ and OCDQ were closed-system instruments.

Hemphill, Griffiths, and Frederiksen. Their study carried the title, *Administrative Performance and Personality*, although it was better known as the "in-basket study." Even though two of the researchers had published theories of administrator behavior, the in-basket study was marked by a strong empirical orientation.[16] The researchers were convinced that all the theories being used in educational administration were inadequate and could not support a major inquiry such as that which was contemplated. While there were few and inadequate theories, there were also few facts with which to work. The researchers, therefore, designed a study to make observations of administrator behavior and relate these observations to as many variables as possible in order to gain a greater understanding of the observations. The fact that the study used the in-basket technique did reflect the belief of Hemphill that administration was problem solving and that of Griffiths that administration was at center a decision-making process. In this sense, the study was influenced by the two theories as it was by many others.

The only direct relationship of the study to logical positivism was that every effort was made to develop operational definitions for concepts employed in the study. The research team designed the in-basket items to include the community and its influences on the school. In this sense, an open-system model was used.

Summary of the Past

A reasonable conclusion to be reached from a review of the prevalent philosophy of science and four prominent studies of the late 1950s and

the early 1960s is that they were little related. Although the prevalent scientific philosophy in the rhetoric of the day was logical positivism, no study or line of studies was done completely in the positivist mode. Getzels and Guba, and Gross, Mason, and McEachern came the closest, but still fell far short. Halpin, and Hemphill, Griffiths, and Frederiksen did not even try. All, however, attempted to define their concepts operationally, treated administration as a rational enterprise, and in varying degrees, sought to develop universal concepts and theories that would hold for any and all organizations.

In retrospect, they all appear to have been practical researchers who took what they thought to be most useful from positivism. The elements that appeared to be impractical—namely, logico-mathematical deduction of hypotheses and the theoretical-observational dichotomy—were ignored. This, however, created a bind in logic. The procedures used were those that the positivists claimed led to verification, but since they did not follow the procedures substantively, they were actually not verifying their theories. As Willower has pointed out, "even though a hypothesis may share a concept with a body of theory, unless the hypothesis is derived, that is, deduced from the theory, evidence on the hypothesis is not evidence on the theory."[17] The logic bind was not recognized at the time. As far as using open- or closed-system models, the trend was definitely toward open-system ones.

THE PRESENT

Whereas it was relatively easy to identify the dominant philosophy of science in the 1950s and early 1960s, it is at first glance very difficult to find a dominant philosophical mode in the present period. There is the acknowledgment that logical positivism is badly flawed, but there is not a clear sign that a post-positivistic approach has been developed. A great many alternatives are being proposed, such as phenomenology, ethnomethodology, symbolic interaction, neo-symbolic interaction, and the sociology of the absurd. Probably the only assumption shared by these approaches is "that each sees human beings as creating, or constructing, their own social reality in interaction with others."[18] Then, too, there is a variety of Marxist approaches that appeal to many social scientists.

The literatures of organizational theory and the philosophy of science contain a wide array of philosophical, scientific, metaphysical, and epistemological views. Although this diversity is reflected in the writing on educational administration, there is an interesting dichotomy. In the United States, the positivist view is strongly presented in the most widely used textbooks. Monahan and Hengst in their recent book, for instance,

use the Fiegl definition of theory.[19] Hoy and Miskel present administrative theory rooted in the theory movement of the 1950s and 1960s—a view that is apparently now considered to be "traditional" and that appears to be the modified positivism presented in the preceding section.[20] On the other hand, criticisms of positivism and of the traditional approach to theory in educational administration are now appearing. Griffiths described many of the alternatives to the traditional mode of theory building and provided a critical analysis of them and of positivism.[21] The criticisms range from the fact that organizational theories ignore the presence of unions and fail to account for the scarcity of women and minorities in top administrative positions, to the failure to account for the degree of external control that is exercised over practically all organizations, and they include the notion that organizational theories strive to be universal but end up with only limited applicability. Even the rationality of organization is now being questioned. Alternatives to the traditional types of theory discussed by Griffiths included phenomenology, ethnography, Marxism, and some extensions of functionalism.

The current situation in the British Commonwealth is somewhat different. Although the volume of literature on educational administration is quite small, it is heavily oriented to a critical view of traditional theory. A recent article by Bates (a New Zealander working in Australia), for instance, listed 73 references, of which 40 were written by Commonwealth scholars and, in many cases, were published in the United Kingdom, Canada, Australia, and New Zealand.[22] The majority of these articles are violently opposed to traditional theory and supportive of alternative approaches. Bates concluded his article by stating that "the normative, social and political dimensions . . . are not matters that can be encapsulated in a behavioral science of administration."[23] At one end of the present continuum is the belief that the effort to study administration and organizational life through the behavioral sciences is nonsense, and at the other end is found the supporters of traditional theory. The struggle, at times, assumes highly emotional levels. As one reads and listens to Bates and Greenfield, one gains the impression that they consider advocates of traditional theory to be idiots at best and pathological at worst.[24] These expressions are often reciprocated. The conflict, as viewed by the two extreme positions, is between those who believe there can be developed scientific theories of educational administration and those who believe there cannot be such theories.

A more comprehensive view of the present scene in organizational theory has been developed by Burrell and Morgan (English sociologists).[25] They base their study on the view that "all theories of organization are based upon a philosophy of science and a theory of society."[26] The authors directed themselves from the beginning of their work toward clarifying the confusion that now exists in social theory, including organizational theory.

> *Our proposition is that social theory can usefully be conceived in terms of four key paradigms based upon the nature of social science and the nature of society. The four paradigms are founded upon mutually exclusive views of the social world. . . . With regard to the study of organizations, for example, each paradigm generates theories and perspectives which are in fundamental opposition to those generated in other paradigms.*[27]

The first strand used by Burrell and Morgan is called either "subjective-objective" or "German idealism-social positivism." The authors raised four basic questions: ontological, epistemic, human nature, and methodological. The answers to the questions lie on continuums. With regard to the ontological question, there are those who believe each person creates his or her own world, while at the other end of the continuum are those realists who believe in an objective world, the same for all, a "social world that exists independently of an individual's appreciation of it."[28] When one considers epistemology, one finds the positivist who seeks regularities and causal relations, as contrasted with the antipositivists, who believe that social science is essentially subjective and that there are no laws or underlying regularities in the social world. In the matter of human nature, the continuum ranges from those who believe in free will to those who believe people are controlled by their environments. The methodological continuum ranges from the idiographic view of social science—that is, only firsthand knowledge counts—to the contrasting view that research should be conducted through the use of systematic protocols and other techniques.

The second strand is the "regulation of order view of society" versus the "radical change view." Burrell and Morgan contrast the two views in Table 1.[29] The two dimensions form four paradigms: the radical humanist, the radical structuralist, the functionalist, and the interpretive.[30] (See Figure 1.)

Virtually all of the theory taught and used in educational administration is in the cell called functionalist organizational analysis (the lower

TABLE 1
The Regulation-Radical Change Dimension

The sociology of regulation is concerned with:	*The sociology of radical change is concerned with:*
a. The status quo	a. Radical change
b. Social order	b. Structural conflict
c. Consensus (voluntary and spontaneous)	c. Modes of domination
d. Social integration and cohesion	d. Contradiction
e. Solidarity	e. Emancipation
f. Need satisfaction	f. Deprivation
g. Actuality	g. Potentiality

SOURCE: G. Burrell and G. Morgan, *Sociological Paradigms and Organizational Analysis*. Copyright © 1980 by Heinemann Educational Books Ltd. Reprinted by permission.

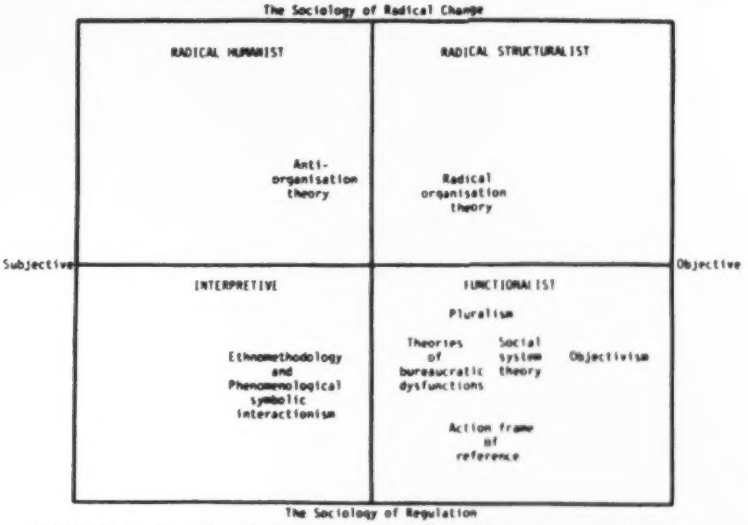
right hand cell). There are three sources of theories in this cell: practicing administrators (as, for example, Barnard), sociologists of organizations (such as Weber), and behavioralists (as exemplified by Simon). The characteristics of the theory are regulation, objectivity, status quo, consensus, social integration, solidarity, need satisfaction, and actuality. Theories tend to be realist, positivist, determinist, and nomothetic. Some examples of theories that are functionalist and are used in educational administration are Social Systems (Getzels-Guba), Decision Making (Griffiths), Systems Theory, Equilibrium Theory (Barnard-Simon), and Contingency Theory.

The interpretive paradigm is based on the belief that people build the social world in which they live. It is in the tradition of German idealism and has its roots in the work of Immanuel Kant. As Burrell and Morgan pointed out,

> *Such a view does not allow for the existence of "organisation" in any hard and concrete sense. Whilst certain schools of thought accept the concept of organisation and its use as an "acccounting practice" by which people attempt to make sense of their world, they do not recognize organisations as such. From the standpoint of the interpretive paradigm, organisations simply do not exist.*[1]

Studies of organizations within the interpretive paradigm are relatively rare, and Burrell and Morgan discuss only efforts utilizing ethnomethodological approaches and phenomenological interaction. They distinguish between the two as follows:

> *Whereas the ethnomethodologist usually focuses upon the way in which individual actors account for and make sense of their world, the phenomenological symbolic*



SOURCE: G. Burrell and G. Morgan, *Sociological Paradigms and Organisational Analysis*. Copyright © 1980 by Heinemann Educational Books Ltd. Reprinted by permission.

Figure 1: The Main Schools of Organisational Analysis

> ***interactionist focuses upon social contexts in which interacting individuals employ a variety of practices to create and sustain particular definitions of the world. They demonstrate how "reality" and "facts" are essentially social creations, negotiated through the interaction of various competing themes and definitions of reality.***[32]

Gephart's study, "Status Degradation and Organizational Succession: An Ethnomethodological Approach,"[33] and much of the December 1979 issue of the *Administrative Science Quarterly*,[34] which was devoted to qualitative method, are examples of the ethnomethodological approach. The article by Pettigrew included in the above issue is the only example of a study done in educational settings.[35] The *Educational Administration Quarterly* apparently has yet to carry an ethnomethodological study, and this writer was unable to find a study anywhere in educational administration that was done in the phenomenological symbolic interaction mode.

The radical humanist paradigm is based upon radical humanism and has its roots in the French existential movement (Sartre) and German idealism (Kant, Fichte, and Hegel). According to Burrell and Morgan,

> *The interpretive and radical humanist paradigms are both founded upon the notion that the individual creates the world in which he lives. But, whereas the interpretive theorists are content to understand the nature of this process, the radical humanists subject it to critique, focusing upon what they regard as the essentially alienated state of man.*[36]

Burrell and Morgan observed that, as of 1979, no one appeared to be studying organizations in the radical humanist mode, but if anyone does, the result will be "*anti*-organisation theory."[37] Their conclusion:

> *Since the radical humanist perspective stands in fundamental opposition to that of the functionalist paradigm, reflecting a complete inversion of assumptions about the nature of science and society, anti-organisation theory will stand in fundamental opposition to contemporary organisation theory.*[38]

The radical structuralist paradigm has provided the framework for a vigorous critique of functional structuralism and, therefore, most of present-day organizational theory. Although there are two strands to radical organizational theory, Marxist and Weberian, the Marxist approach is far more popular. The Marxist critique includes a number of variations called neo-Marxism, ethno-Marxism, and the Marxist dialectic. The Marxist view interprets organization life in terms of power, conflict, contradications, crises, dialectics, and class; and this type of interpretation is the basic thrust of the Marxist critique of traditional organizational theory.

In summary, the present era is characterized by an increasing volume of research by functional structuralists, while at the same time the rate of criticism of the functional structuralists by scholars of the interperetive, radical humanist, and, especially, the radical structuralist persuasions is also increasing. What initially appears to be chaos now appears to be quite understandable. Morgan has described the present as a time of "paradigm diversity,"[39] and, indeed, it is.

Major Research Studies

The present is not noted for the large, well-funded studies that marked the 1950s and 1960s. More typical is the development of lines of inquiry by a single professor working with graduate students. The four prominent sets of researchers discussed in this section are good examples of such activity.

Bridges. Although the research detailed in "Job Satisfaction and Teacher Absenteeism" does not carry names, it seems from the

description of procedures that there were others involved with Bridges.[40] The study began with assumptions that might possibly account for teacher absenteeism. No clean theory can be discerned from the discussion. Bridges then stated that "the foregoing discussion *suggests* four alternative hypotheses."[41] A variety of data—some self-reported perceptions and some objective such as salary, sex, and age—were gathered. Twelve different multiple regression analyses were then used to estimate the relationship between job satisfaction and absenteeism. Only one of the four hypotheses received support.

The methodology followed the traditional (that is, modified positivist) approach to research. A rather vague theory was developed and hypotheses were suggested. The hypotheses were tested with both theoretical and objective measures. The study apparently was conducted in the context of closed-system thinking. Some of the concepts were operationally defined. It would appear that this study was done in a loosely constructed traditional mode.

Gross, Giacquinta, and Bernstein. The research detailed in *Implementing Organizational Innovations* is a case study of an elementary school attempting to implement a major educational innovation.[42] Gross et al. used the case study approach because

> *it provided a strategic way to explore the complex organizational problem we proposed to study; it permitted us to carry out in-depth observations of the several aspects of the dynamics of an effort to institute planned change into an organization; it also made provision for the use of a variety of data-gathering methods, a desideratum for our inquiry.*[43]

Fieldworkers observed the effort to change the role of teachers in an elementary school. They employed formal and informal interviews, classroom observations, self-administered questionnaires, analysis of documents, and informal observations of teacher behavior. Six months after the innovation was introduced, it was not implemented—a fact that the researchers explained by deriving five circumstances from their observational data. They then critiqued current theories of change using the circumstances as their basis and finished by setting up a new theoretical framework. The study concluded with suggestions of questions to be studied and implications for practice, both derived from their theory. As Gross et al. stated, "Case studies, of course, can generate, but do not test, theories."[44]

This study was not in the positivistic mode but, rather, was an effort to point out weaknesses in current theories of change. These theories

were not tested in the traditional mode—that is, by deriving hypotheses and testing them through the theoretical-observational dichotomy. The study represents an effort to break out of the positivistic approach in theory development. However, it is quite clear that the goal of the study was to build theory from which hypotheses could be deduced and verified.

Miskel, Glasnapp, and Hatley. The research approach used by Miskel varied from those described above.[45] In some studies, he developed a "theoretical framework" that is a combination of theories, concepts, opinions, and personality characteristics.[46] These were described and followed immediately by a statement of hypotheses. In other studies he developed a "rationale," and from this, hypotheses were "drawn."[47] In combining the various components of the theoretical frameworks and rationales, apparently little or no attention was paid to the comparability of the components. Certainly there was no attempt at theory reduction in the positivist mode.[48] What Miskel seemingly did was to accumulate a number of ideas in which he was interested and, by intuition, to arrive at some hypotheses. The hypotheses were then tested using questionnaires and various types of psychological instruments. The statistical procedures used were quite advanced.

It is difficult to characterize the research approach of Miskel and his colleagues. At first blush it appears to be hypothetico-deductive in the positivist tradition. However, he did not deduce hypotheses from theories, and he did not employ the theoretical-observational dichotomy. Further, it cannot be said that his approach was successful (which would fulfill Putnam's criterion for science), since so few of his hypotheses were fully supported.[49] The five studies reviewed for this analysis included eleven hypotheses, of which Miskel reported one to be fully supported and ten as being partially supported.

Willower, Eidell, and Hoy. One of the better-known recent studies is *The School and Pupil Control Ideology*.[50] Although a number of papers have been published on the pupil control research, only one is referred to here since it is the basic report. The conceptual scheme for the studies was based on social systems theory, a line of inquiry Willower has followed since his doctoral student days. The studies investigated the relationship between the way teachers and principals view pupil control and what kind of a school social system results. Rokeach's concepts of open and closed mindedness were employed in order to understand more fully the attitudes of school personnel.

Hypotheses were deduced (it is not clear as to how they were deduced, however) and tested with a number of psychological instruments. All of

the predictions about client control ideology were supported. Willower et al. were apparently more successful in generating hypotheses that were supported than were Miskel et al. and Bridges because the former stayed close to social systems theory—a thoroughly studied line of inquiry.

Willower's concepts were defined operationally, and the study was based on a closed-system model. The same methodological comments can be made about Willower et al. as were made about Bridges and Miskel et al.—namely, it is not clear how the hypotheses were derived, and the observational statements were comprised largely of theoretical concepts.

Summary of the Present

All of the theories used in the research studies described as having been done by prominent researchers in the present are what Burrell and Morgan categorized as functionalist organizational theory. All of the theories employed tend to be positivistic in that the theorists are concerned with regularities and causal relationships, but there is little else of positivism in the theories. Bridges and Miskell et al. had even more of a problem with the generation of hypotheses than did the researchers of the past. Hypotheses are "suggested," or "drawn," but apparently not deduced. The theories are of the realist persuasion in that they hold for an objective world independent of individual perceptions. The theorists assume that the behavior of members of organizations is determined by the environment of the organizations and that the members do not have free will. While they attempt to account for individuals, the theories are centered on the organization; they are nomothetic. They all hold that organizational behavior is rational and goal oriented.

The studies by Bridges, Miskel et al., and Willower, Eidell and Hoy appear to employ closed-system models, whereas Gross, Giacquinta, and Bernstein had an open-system view of the school. The use of closed-system thinking is particularly difficult to understand when considering the pupil control focus of Willower; it seems that most observers of the public schools credit the community and family with much of the blame for pupil control problems. Likewise, in Miskel's work it is difficult to understand how work motivation, principal's effectiveness, and job satisfaction can be adequately studied with closed-system models. Miskel apparently thought that it makes no difference if a school is in Scarsdale or the South Bronx. Bridges employed a closed-system approach in studying the relationship between job satisfaction

and absenteeism. He concluded that his study suggested that job satisfaction is not a major factor in absenteeism. Possibly the recognition that schools are more open than closed would help in conceptualizing the problem. As Hoy has pointed out in his review of the literature, "Indeed, there is now a general consensus that modern organizations, including educational ones, are open systems."[31] It would appear that the prominent researchers of the present, with the exception of Gross et al., are still concerned with schools as closed systems, even though the prevailing orientation is to view schools as open systems.

There seems to be less obvious concern with operationalizing concepts than in the earlier period, although it is clear that many of the concepts must have been so defined.

At the present time the writers of the popular textbooks in educational administration use a strictly positivist definition of theory, the theorists are completely at odds as to what theory is, and the prominent researchers ignore both and attempt to come up with useful knowledge.

CONCLUSIONS

The purpose of this article was to attempt to determine if there was an evolution in research and theory from the middle 1950s to the present time. The method used was to study the work of prominent researchers in both periods of time. It was not the intention to claim that the work of prominent researchers was typical. Rather, it was the best work being done in both time periods, and it is likely that this work set the pace for less-well-known researchers.

Research

The research methodology of the two periods is similar. The study of Whitman School in the in-basket research was more of an in-depth case than was Gross's study of innovation in the Cambire School. However, they both employed observers and interviewers in the actual setting. The studies of Getzels, Gross et al. (superintendency study), Bridges, Miskel et al., and Willower et al. are quite similar in methodology. The disparity is that Getzels, Willower, and Gross were always clear about where they were coming from; their theory was explicit. The two researchers in the present period employed theoretical frameworks and rationales that were combinations of theories, models, concepts, research fidings, and opinions. It is only by a long stretch that the

resulting framework can be called theory. Again, it is easier to understand how Getzels, Willower et al., and Gross et al. arrived at their hypotheses; it is not at all clear how Bridges and Miskel arrived at their hypotheses. It is obvious, as a generalization, that both past and present researchers worked within the same philosophy of science mode. However, the present researchers work more loosely within the form of logical postivism.

Theory

Both the past and present researchers can be said to be working within structural functionalist theory. Aside from being less rigorous at present, there is really little difference between the two sets in the nature of the theory they espouse. Getzels and Gross (superintendency study) were explicit as to their theory, described it carefully, and used their research to modify the theories. It is not possible to state clearly the theory or theories of Bridges and Miskel. The latter reflect the condition in science generally, as typified by Shapere: "There is today, no completely—one is almost tempted to say remotely—satisfactory analysis of the notion of a scientific theory."[52]

The past researchers deviated from the dominant philosophy of science espoused in their times, which was positivism. Neither Halpin nor Hemphill et al. started with a theory, derived hypotheses, or in any major way functioned in the positivist mode. Getzels and Gross (superintendency study) were close to the positivist approach, but deviated in practical ways as noted above.

Today's prominent researchers reflect the dominant theory of the times—functionalism—and they are fully within its strictures. Their work, however, does not reflect any recognition of the criticisms of functionalism or any of the newer philosophical approaches now appearing in the literature. Since, however, the newer approaches have different assumptions, it would be difficult, if not impossible, to do research using both functionalism and antifunctionalism simultaneously.

There were, in the late 1950s, a limited number of theories in educational administration: social systems, role, and decision making. They were positivistic and constituted the core of traditional theory. At the present, the situation is very different. It would be difficult to list all of the alternatives to traditional theory, but without trying to be inclusive, some of them are as follows: Marxism, ethno-Marxism, phenomenology, ethnomethodology, symbolic interaction, the "new sociology," and the sociology of the absurd. In addition, such critics as

Bates and Greenfield contend that there cannot be theories of educational administration in any scientific sense. From a situation, only 25 years ago, in which the field seemed reasonably confident that it had a small number of satisfactory theories, we have now reached the condition in which there are numerous theories, and many are contending there should be none. The majority of scholars in educational administration, however, still hold for traditional theory.

Comments

When comparing research and theory past and present, it seemed that the philosophy of science espoused was always more sophisticated than the research being done. Recall, for instance, Halpin's paper in *Administrative Theory in Education*, in which he proposed the Fiegl definition of theory as the acceptable mode for the research going on at the time.[53] No one in educational administration ever fully used this definition in their research, nor did they do their research completely within the mode being advocated. Likewise, at the present time, there is a large amount of criticism of functionalist theory, and a number of new approaches are being advocated. Boyan, however, in reviewing research found no studies employing either phenomenology or Marxism. [54] This writer has been unable to find more than one study using ethnomethodology, and none in such approaches as the "new sociology." There appears to be a gap between the philosophers of science and the theoreticians on the one hand, and the prominent researchers on the other.

When comparing past and present researchers in the same mode — namely, Getzels and Gross (superintendency study) as compared with Bridges, Miskel et al., and Willower et al. — a significant difference in style of working was noted. Getzels and Gross each worked with a single theory: social systems and role theory, respectively; whereas the present researchers work with composites or aggregates of theories, models, ideas, and opinions. The present researchers are not as concerned with the form of research as were their predecessors. They are more in line with what appears to be the prevailing opinion in all of the sciences as expressed in the book *Men of Ideas*, a set of dialogues with Magee:

> *Magee – But now that our whole conception of science has altered, it is not so much that some different method has come to be regarded as "the" method, but rather that there is no longer thought of as being only one single valid approach to all scientific problems.*

Putnam—There is a sort of paradigm of "the scientific method" (a paradigm which itself is pretty vague, as I just remarked) which one occasionally finds pretty well exemplified, especially in physics. But even in physics there is a great deal of knowledge which doesn't, and shouldn't, fit the paradigm. I don't believe that there is really an agreement in our culture as to what is a "science" and what isn't. Any university catalogue claims that there are subjects called "Social Sciences," and that Sociology and Economics are sciences. But I would bet that if we asked anyone in the Physics Department whether Sociology is a science he would say "No."

Magee—Yes, but what grounds would he have for saying no?

Putnam—I think the real reason is not that the sociologists don't use the inductive method—they probably use it more conscientiously, poor things, than the physicists do. I think it's because they're not as successful.

Magee—So you think "science" is now simply a term for the successful pursuit of knowledge?

Putnam—That's right.[33]

Scientists now are much more concerned with the successful pursuit of knowledge than with the form of research, and this might well be what is happening in educational administration at the present time. Far more emphasis is being paid to whether or not useful information is being generated than to following a particular form of inquiry. In this sense, the prominent researchers in educational administration are working in modes similar to those in the physical sciences.

Finally, the term "theory" appears to have little or no meaning at the present time. Theory is such a nice term. It might be worthwhile to try to return it to respectability.

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5 Can there be a science of organisations?

Daniel E. Griffiths

Four years ago I gave the invited address at the 1979 Annual Meeting of the American Educational Research Association (AERA) at San Francisco (Griffiths 1979a). In that paper I listed five steps which I said must be taken in the face of our present dilemmas:

- An attempt should be made to answer the question: Can there be a science of organisations comparable to the science of physics or chemistry?
- Efforts to develop a general theory of organisations should be replaced by efforts to develop much more restricted theories.
- We need to begin to rethink the conceptual bases of organisational theory.
- We need to try to answer the question: Just what is there in the research and theory on organisations that is useful to us in educational administration? (Silver 1983 makes a successful effort to answer this question.)
- Theories should be built which give a much larger place to the environment, to networks of related organisations, to the goals of others than the executives, to unions, and, in higher education, to professors as managers. The theories should be closely related to the socio-cultural context so as to explain the place of women and minorities in organisations.

Attention has been paid to some of the points I made, not because I made them, but because these ideas are 'in the air'. (See, for example, Willower 1975, 1980, 1983; Burrell & Morgan 1979; Morgan 1980; Hoy 1978; Bates 1982). In this paper I am joining those such as Willower, Hoy, Bates and Greenfield, who have addressed the subject, to try to answer the question: 'Can there be a science of organisations comparable to the science of physics, chemistry and mathematics?'. Note that I am not posing the question: 'Should there be scientific theory in educational administration?'. Nor am I posing the theoretical vs. practical argument. I am raising the question in terms of whether it is possible to develop a scientific theory in the sense of theory in mathematics and physics. If it is not possible to have such theory, what kind is possible?

The issue I wish to explore first is, I think, best understood by presenting adversarial views. Bates (1982, p. 12) concludes a recent article by saying: 'The normative, social and political dimensions of such a struggle [in the interest of a better world] are not matters that can be encapsulated in a behavioural science of administration'. While this statement is couched rather narrowly, in his paper Bates (1982, p. 2) goes well beyond this narrow conclusion and brands the behavioural science approach to organisational theory as 'pathological'.

Going even further is Greenfield (1978), who concludes, after an examination of what appear to be the assumptions of ethno-Marxism, that the assumptions lead to '. . . not a new theory or organization, but a different way of looking at organizations'.[1] He goes on to say: 'Within these assumptions there probably cannot be *a theory* of organizations' (Greenfield 1978).

On the other hand, Hoy, in his Presidential Address to the Plenary Session of the University Council for Educational Administration (UCEA) in February 1978, made

a ringing affirmation of a commitment to theory-based, scientific research stating: '. . . I believe educational administration, as a discipline, can best preserve its own uniqueness by reaffirming its commitment to scientific research' (Hoy 1978, p. 1). By scientific research he means '. . . the systematic and critical empirical investigation of hypothetical propositions' (Hoy 1978, p. 1). Further:

> The practice of science is a way of knowing things and requires a faith that it is a valid approach to knowledge and an assumption that the nature of reality is ultimately material and knowable. Practiced rigorously and honestly, scientific research can be embedded in any culture or any time and produce reliable findings (Hoy 1978, p. 3).

Willower (1980, p. 2) is equally direct and unequivocal in what he believes: 'Theories in educational administration have to be seen in the context of the social sciences and science itself'. Willower supports, however, Merton's (1968, p. 52) view of a large amount of sociological theory which '. . . consists of general orientations toward data, suggesting types of variables which theories must somehow take into account rather than clearly formulated, verifiable statements of relationships between specified variables' (quoted in Willower 1975, p. 79). Willower holds the position that the notion of orientations towards data is loose enough to accommodate much of the theoretical work in educational administration.

So we have the present situation: Greenfield contending that because of the assumptions he holds there can be no theory; Bates apparently believing that theory can be developed, but not with a base in behavioural science; Hoy holding as closely as possible to logical positivism in his theoretical outlook; and Willower staunchly advocating theory based on social science and science. It should not be thought for a moment that each stands alone. They do not, for each has a substantial number of followers.

Let us look closely at these views. First, I believe that Willower, as usual, analysed the present condition in our field in a very succinct fashion in his paper prepared for this conference when he said: 'In educational administration and in administrative science, considerable mystification has resulted from the division of the world into an extreme version of positivism on the one hand, and whatever view is favored by the writer on the other' (Willower in press). Greenfield (1975) started this trend at the International Intervisitation Programme (IIP '74); and Bates (1980, 1982), among others, has been an even more enthusiastic practitioner. Bates (1982, p. 2) paraphrases Greenfield as saying, for instance, 'A theory of educational administration that divorces fact from value, theory from practice, rationality from commonsense, and education from administration is unlikely to be capable of guiding the administrator's hand'. I certainly agree with the statement. But who ever put forth such an asinine position? Certainly no one I know supports such a theoretical posture. Bates (1982, p. 2) put 'traditional theorists' all in one box as follows: 'Despite the social theorists' large scale abandonment of the quest for a value-free science of society the mainstream theorists of educational administration still pursue positivistic attempts to develop generalisable laws and principles which will explain the structure and dynamics of (all?) organisations'.

I have just finished a minor study in which I examined the work of four sets of prominent researchers in the late 1950s and four sets of prominent researchers of the modern day. I also studied the prevailing theoretical mode of both periods (Griffiths 1983). None of the researchers, present or past, practised science as the positivists said it is and should be practised. On the other hand, the theoretical line, the ideology if you will, in both periods was positivist. The researchers were doing their work as best they could without adhering to any one philosophy of science approach. What is necessary, apparently, is to look at what people do, rather than what they say.

What, then, is Bates doing? First, he denounces all 'mainline theorists' in educational administration calling them positivists who develop 'pathological' theories (Bates 1982). Then he invokes phenomenology as the antidote and promptly launches into the 'new sociology' which is clearly a Marxist recipe for administrators—and, I must say, not a very good one at that. Phenomenology is no sooner mentioned than forgotten. While the argument is that there can be no behavioural science-based theory for educational administration, he presents no evidence that this is so. The closest he comes is to again paraphrase Greenfield: 'Essentially, Greenfield argues that far from excluding personal values and beliefs from the scientific analysis of organizational behavior, it is impossible to understand organizations without taking them into account' (Bates 1980, pp. 7–8). Again, I agree completely, but so does everyone else. In the study I just completed, I examined the work of Getzels, Halpin, Hemphill, Griffiths, Frederiksen, Gross, Miskel, Willower, and Bridges. Every one of these studied the personal beliefs and values of the people in organisations. In fact, virtually all of the studies attempted to relate all sorts of psychological measures to other variables. But Bates's essential argument is against a 'behavioral science of educational administration'. A quotation gives the flavour of his affection: 'Though misconceived and misdirected the quest for a behavioural science of educational administration continues' (Bates 1982, p. 1). But what does Bates suggest in its place? He advocates what is called 'the new sociology' and argues that '. . . what counts as knowledge is closely related to the interests and power of social groups. What counts as knowledge in differing groups is different, but what counts as knowledge in schools and formal educational systems is determined largely by the interests of the powerful' (Bates 1980, p. 9). Bates's involved argument runs as follows:

1 What counts as knowledge in social life is determined by the nature and structure of that social life.
2 The structure of social life is determined by the means of production.
3 The social life of any group is therefore differentiated according to the division of labour.
4 The division of labour (at least in capitalist society) leads to the creation of differences in power, thus leading to the creation of a hierarchy and the development of an elite.
5 The exercise of power by the elite both determines and is determined by the division of labour which is a result of the means of production which controls the social life and consciousness of men which is what counts as knowledge (Bates 1978, p. 318).

I have two points to make concerning Bates's argument. The first is very simple: it is an unbelievable argument. The powerful, in my opinion, do not give a damn about what is taught in schools and universities; and the more powerful they are, the less they care. Bates, of course, never says who the powerful are, so it is fair game for the reader to put his notion of the powerful into the argument.

When I put forward this critique at Rutgers it evoked, much to my surprise, a violent reaction. Many in the audience contended that the powerful do, indeed, determine what counts as knowledge in schools and formal educational systems. What the groups appeared to be saying, however, was that the powerful do influence schools, largely through controlling resources, and they confused this with knowledge. The powerful do, at times, attempt to create knowledge to conform to their own interests. 'Creationism' is one such example at present. Another example is the concept of the transmission of acquired characteristics. Lysenko proposed the notion in Russia and received support from the Soviet hierarchy, since it was very much in the interests

of Soviet leaders to promulgate a doctrine in which their leadership skills would be thought to be passed on to their sons. When it was obvious that the acquired characteristics were not transmitted to their sons, the concept was discredited. Professor van Fraassen, in a discussion of this point, observed that 'science has ideological roots only in a harmless sense'. To elaborate briefly, virtually anything can be proposed by the powerful (or anyone else), but if there is no justification, there is no knowledge.

The second point is, I believe, more to the topic. Note the concepts Bates uses: 'social life', 'production', 'labour', 'capitalist society', 'power', 'hierarchy', 'elite' and 'division of labour'. Every one of them is a behavioural science concept! What Bates has done is to construct a behavioural science theory for the selection of curriculum; and while it is not believable, it might be welcomed by our colleagues in the Association of Supervision and Curriculum Development (ASCD). Bates's case against a behavioural science base for theory in educational administration is not a compelling one.

On the other hand, Greenfield, when he speaks for himself, makes quite a different argument. As you recall, he holds that when his assumptions are applied there cannot be a theory of organisations. What does he mean? First, he might mean that the alternatives to positivism, such as action research and theory of practice and such approaches as ethno-Marxism, all offer interesting and often useful information about organisations. These, and similar approaches, also frequently offer individuals an interpretation of their own experiences. But what is learned in these ways is not scientific knowledge—it is not generalisable, and often is of little value to anyone other than the individual using the approach. These alternatives to positivism are not scientific theories.

A second way of interpreting Greenfield's argument is, I think, quite significant. Here I am speculating because Greenfield does not speak clearly on this issue; but even so, when he raises the question of what constitutes a scientific theory, he is talking to a point discussed at length in the literature of philosophy of science and in other fields such as aesthetics.[2] I have discussed this question in detail in a previous article and shall only recapitulate the discussion here (Griffiths 1979b). The argument that there cannot be a scientific theory of organisations stems from the work of Wittgenstein whose two books (virtually unintelligible to the layman and most others) sparked a revolution in philosophy. (Wittgenstein was a student of Bertrand Russell, a leading positivist, and the story is that Russell wrote a glowing introduction to Wittgenstein's doctoral dissertation, *Tractatus*, first published in English in 1922. It was not until some time later that Russell discovered that the *Tractatus* was the first nail in the coffin of logical positivism.)

While Wittgenstein, of course, never discussed organisation theory (nor do his philosophical disciples), he did speak to our general concerns. To quote part of paragraph 6.53 from the *Tractatus* (1971, p. 151): 'The correct method in philosophy would really be the following: to say nothing except what can be said, i.e. propositions of natural science . . .' Wittgenstein's biographers elaborate on this by saying: 'There simply cannot be any meaningful propositions except those of natural science . . .' (Janik & Toulmin 1973, p. 199). This might be interpreted to mean that a physical science-type theory of organisations is impossible.

However, the aphorism concerning propositions of natural science was enlarged upon by Wittgenstein in *Philosophical Investigations* (1953). He gave as an example the concept 'games' as typical of a set of concepts that could not be rigorously defined. He opened his discussion by raising the question: 'What is a game?'. He then asked us to look at all sorts of games and consider what is common to them:

> Don't say: 'There *must* be something common, or they would not be called "games"'—but *look and see* whether there is anything common to all.—For if you look at them you will not see something that is common to *all*, but similarities, relationships, and a whole series of them at that (Wittgenstein, Part 1, 1953, p. 31, para. 66).

This analysis was picked up by Weitz (1970, 1977). Weitz (1970, p. 175) reasoned that 'game' was an *open concept*, and argued that a concept is open if 'its conditions of application are emendable and corrigible; i.e. if a situation or case can be imagined or secured which would call for some sort of *decision* on our part to extend the use of the concept to cover this, or to close the concept and invent a new one to deal with the new case and its new property'. He contrasts the open concept with the *closed concept* in this way: 'If necessary and sufficient conditions for the application of a concept can be stated, the concept is a closed one' (1970, p. 175). But this can happen only in logic or mathematics where concepts are constructed and completely defined. It cannot occur with empirically descriptive and normative concepts unless we arbitrarily close them by stipulating the ranges of their use (Weitz 1970). Whereas *game* is an open concept, a particular game can be a closed concept. Weitz (1977, p. 39) illustrates this in a homey way by saying: '"Major-league baseball" . . . unlike "game", though fluid and flexible enough in its history, can perform its assigned role under the overall condition that it is governed by a set of definitive criteria so fixed as to ensure that no unresolvable doubts and disputes can arise'.

It seems to me that the concept 'organisations' is similar to the concept 'games'. If we were, following Wittgenstein, to ask the question, 'What is an organisation?', we would then look at all sorts of organisations—a one-room schoolhouse in the Maine woods or the Australian outback, and Cambridge or Princeton; a two-man machine tool business and IBM; MacDonald's and the Conought Grill Room—to see whether there is anything in common. Aside from the fact that there are people in all the examples, we will not find much in common between the one-room school and Cambridge, and nothing in common between the one-room school and IBM. (For a discussion of this point, see Campbell in press.) You will not see commonalities, but similarities, relationships and whole series of them at that. 'Organisations' is, therefore, an open concept and can only be closed by stipulating the range of its use. I have no compunction about arbitrarily defining 'organisations' to cover a narrow range of institutions much in the manner of Weitz's use of major-league baseball.

So I say that Greenfield's observation is a useful one because in examining it we must become more precise in our thinking. However, I do not think the argument from Wittgenstein should be interpreted to mean that there can be no scientific theory of organisations or of educational administration.

I now turn to a discussion of the kind of scientific theory that might well be possible in educational administration, and, in so doing, include some analysis of the positions of Hoy and Willower.

First, let us clear the air as to where we are in educational administration as regards scientific theory. Textbook writers such as Monahan, Hoy and Miskel still use the Fiegl definition of theory, but the researchers (including Hoy and Miskel) are not postivists in their work (Griffiths 1983). In practice, but not in ideology, positivism is as dead in educational administration as it is in the natural sciences.

Next, a few words as to what is meant by science and scientific theory, and what it is we want theory to do. I like what Snow (1971, p. 56) has to say: 'The scientific process has two motives: one is to understand the natural world, the other is to control it'. For us, I think this means that we need to understand our educational institutions

and that we can control the institutions rather than the institutions controlling us. I also like this advice from Snow:

> Scientists know their kinds of beauty when they see them. They are suspicious, and scientific history shows they have always been right to have been so, when a subject is in an 'ugly' state. For example, most physicists feel in their bones that the present bizarre assembly of nuclear particles, as grotesque as a stamp collection, can't possibly be, in the long run, the last word (Snow 1971, p. 190).

In this same sense, administrative theory is 'ugly'. The widely diverse methods being advocated, the lack of power in the variables we use, the insignificance of our findings, all lead us to the conclusion that where we are cannot be the final stage.

The new views of theory now emerging in the philosophy of science offer some guidelines for us in our theory and research. I find the recent book by van Fraassen (1980) to be useful in that it speaks to many of the issues that we have debated for years and, at the same time, offers an alternative, constructive empiricism, to logical positivism and its immediate successor, scientific realism. While I agree with much of what Hoy has to say about scientific theories, I do disagree, or fail to understand him, on others. Hoy (1978, p. 3) states, 'Practiced rigorously and honestly, scientific research can be embedded in any culture or any time and produce reliable findings'. I take it that what Hoy is saying is that scientific findings of phenomena in one culture hold in another culture; that scientific research, and the resulting theory, is culture free, or is, as now said, context free. This is a view no longer supported, even in the physical sciences. The discussion of this point should be lengthy because it bears on a number of crucial issues, but I shall only allude to them. In his discussion of the importance of context, van Fraassen writes of an investigation into a death resulting from an automobile accident. He states:

> It might be thought that when we request a *scientific* explanation, the relevance of certain hypotheses . . . are automatically determined. But this is not so, for both the physician and the motor mechanic are asked for a scientific explanation. The physician explains the fatality *qua* death of a human organism, and the mechanic explains it *qua* automobile crash fatality. To ask that their explanations be scientific is only to demand that they rely on scientific theories and experimentation, not on old wives' tales. Since any explanation of an individual event must be an explanation of that event *qua* instance of a certain kind of event, nothing more can be asked (van Fraassen 1980, pp. 129–30).

Van Fraassen sums up for us in this way:

> The discussion of explanation went wrong at the very beginning when explanation was conceived of as a relationship like description: a relation between theory and fact. Really it is a three-term relation, between theory, fact, and context (van Fraassen 1980, p. 156).

The importance of context is emphasised thus:

> Nevertheless, in the analysis of the appraisal of scientific theories, it would be a mistake to overlook the ways in which that appraisal is coloured by contextual factors. These factors are brought to the situation by the scientist from his own personal, social, and cultural situation. It is a mistake to think that the terms in which a scientific theory is appraised are purely hygienic, and have nothing to do with any other sort of appraisal or with the persons and circumstances involved (van Fraassen 1980, pp. 87–8).

Thus far I have tried to make these points:

1 Scientific theories must be based upon closed concepts. Therefore, the concept 'organisations' must be narrowly and completely defined and, in being so defined, will be a very limited concept.
2 The context of an inquiry is of crucial importance and it cannot be assumed that a finding in one context holds in another.

My second disagreement with Hoy is a very recent break. Until very recently I agreed with him on his contention that '. . . scientific research refers to the systematic and critical empirical investigation of hypothetical propositions. In this definition, the purpose of research is to develop theory, and, of course, the purpose of theory is to understand and explain phenomena' (Hoy 1978, p. 1). This is fine as far as it goes and is the position of what could be called traditional science. Van Fraassen offers a reverse picture. 'The real importance of theory, to the working scientist', he says, 'is that it is a factor in the experimental design' (1980, p. 73). He discusses the point in this way:

> Scientists aim to discover facts about the world—about the regularities in the observable part of the world. To discover these, one needs experimentation as opposed to reason and reflection. But those regularities are exceedingly subtle and complex, so experimental design is exceedingly difficult. Hence the need for the construction of theories, and for appeal to previously constructed theories to guide the experimental inquiry (van Fraassen 1980, p. 73).

It seems to me that van Fraassen is saying that the role of theory includes more than the testing of hypotheses and understanding and explaining phenomena, as Hoy contends. The role of theory, which is particularly significant in research, is to give guidance in constructing the methods of testing the hypotheses. While van Fraassen uses the term 'experimental design', since he works in the physical sciences, more inclusive terminology is appropriate for those of us working in the behavioural sciences. Once hypotheses are arrived at (preferably by deduction from the theory), the question is, what kind of a study should be designed to test the hypotheses? Should there be an experimental design, a historical study, a case study, a survey? It seems to me that this step is often overlooked in educational administration. Instruments such as the LBDQ and OCDQ or home-made questionnaires have been mindlessly employed to devise tests of hypotheses which are not appropriate to the theory or to the hypotheses. It can be said that in addition to determining hypotheses and understanding phenomena, theory guides the structure of research.

To conclude, I return to the question raised at the beginning of this paper: Is it possible to develop a scientific theory in the sense of theory in mathematics and physics? I think the answer is clearly and unambigiously, no. This does not mean, however, that there cannot be scientific theory in educational administration. After all, there is scientific theory in a number of physical sciences such as biology, zoology, and astonomy which is not as rigorous as mathematics and physics, yet, it is useful and accepted.

It appears that theories can be constructed which have limited generalisability. Efforts, to date, have aimed at universal theories, theories that apply to all organisations in all places. Theories of administration should be limited to specific types of organisations which exist in carefully defined contexts. Campbell's contention (in press) that educational administration is a special case is apparently correct but it appears that there are special cases within special cases. In other words, theories of administration that apply to Ivy League universities most likely have little to do with the administration of inner-city schools. It should be possible to construct theories about administration in certain carefully defined types of organisations.

Notes

1 I here acknowledge the contribution of Professor Greenfield in bringing educational administration into the modern era of intellectual thought. The Griffiths–Greenfield debate is usually misunderstood: it is not a debate on the merits of positivism vs. phenomoneology, but rather a critique of organisational theory from two viewpoints.

2 Greenfield has asserted, in personal correspondence, that demonstrating that there can be no scientific theory of administration is a major theme of his current work. The confusion arises when various of Greenfield's writings are compared. Greenfield's 1978 article contains the statement, 'Within these assumptions there probably cannot be a *theory* of organizations'. Yet his 1980 article has a section which looks suspiciously like a structural functionalist or positivist approach to scientific theory. Although the section is titled a 'prolegomena', it is a set of propositions which Greenfield apparently feels hold for all organisations in all cultures, and at all times. His only disclaimer is that they cannot be confirmed or disconfirmed by empirical facts alone. Further, if there cannot be a scientific theory of administration, why does Greenfield constantly use the terminology, as in 'Organization theory as ideology' (1978), for instance.

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The individual in organization: A theoretical perspective

6

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Whenever a discipline seems to have solved its major problems even more serious ones emerge. There is no time at which one can relax and feel comfortable with the knowledge that has accumulated. Such is the case in educational administration. While, certainly, no group has felt recently that enough was known, there was the belief that educational administration was on the right track. Most, if not all, dissertations employed theoretical concepts and structures, textbooks contained descriptions of several theories, and administrators were taught to make theoretical analyses. In Kuhn's language, there was an accepted paradigm, and the processes of "normal science" were ongoing. Normal science here means the testing of all of the conceivable hypotheses that can be derived from a theory. It also means the tidying up of the theory, what Kuhn calls "mopping up." True, the quantity of normal science was low and the quality variable. However, there was a paradigm, and there was normal science.[1]

CRITIQUE OF THE ACCEPTED PARADIGM

The paradigm consisted largely of the Getzels-Guba social systems model, role theory, decision theory, bureaucracy, and systems theory.[2]

The theories held many assumptions in common. They assumed that organizations have goals that the members strive to attain, that there are roles, sets of expectations for the members that are agreed upon (the nomothetic dimension), that behavior is more or less governed by a set of rules (bureaucratic structure), that decision making is a systematic process, that only legitimate power is employed, and that merit is superior to politics. Administration, organizations, and organizational behavior were viewed as essentially orderly and rational. Organizations functioned regardless of the individuals within them. In spite of the fact that these assumptions were commonly held, it is unlikely that anyone but a committed researcher would consider that such a description typified a modern public school or university. Indeed, there are those who believe the description does not apply to any modern organization.

There are two major approaches by which the paradigm can be critiqued: (1) by consideration of the changing posture of individuals in organizations; and (2) from the viewpoint of phenomenology.

Individuals in Organization

Philip Johnson was recently quoted as saying, "Let us celebrate the death of the *idée fixe*. There are no rules, only facts. There is no order, only preferences."[3] Knowing Philip Johnson, I am certain that he grimaced as he said it, just as I do. However, this observation by one of the world's great architects, uttered in the context of his own field, is applicable to most of modern living. For us as administrators and professors of educational administration it is particularly applicable because our theories have been based on the *idée fixe*.

Lord Morris of Grasmere, an English philosopher and university administrator, has described clearly, concisely, and with remarkable brevity an emerging concept of the administrator in the modern world.[4] The significant element, he said, is this: "The peoples do not want to be governed, and clearly they do not believe that there is any real and final necessity to be governed";[5] and further, "Yet governments must clearly go on trying to govern. And it is against this background that administrators will have to live and work."[6] In terms of governmental leadership, he thinks, "The most that is likely to emerge is a leader who is a genius at forecasting what is practical in government, which means fundamentally, and perhaps exclusively, what is acceptable."[7]

We are living with a new kind of politics and this has its repercus-

sions on all who would aspire to govern and to administer. As Lord Morris put it, "The new Machiavelli can no longer make up his mind what he wants to do and then bring the people round to putting up with it. His primary problem, almost it seems his whole problem, is to find some act of government, or any act of government, which is acceptable."[8]

Men are saying much the same thing in our country. Commager was correct when he said: "There is no consensus. There is less harmony in our society, to my mind, than at any time since, say Reconstruction. Perhaps the 1960s and 1970s are a great divide — the divide of disillusionment."[9]

Robert Nisbet has given considerable thought to the reasons for the collapse of consensus and has expressed them this way:

> *There is every reason for concluding that we are living in one of history's twilight periods; in our time a twilight of politics. It is the fate of all civilizations to outgrow the system of power that binds them. This, quite clearly, is happening in the West today, not least in the United States. I believe the waning of the political order . . . is a fact of highest significance, and far too little noted.*[10]

> *Currents of localism and regionalism, as well as of ethnic, religious, communal, and other particularist values, all generated in such large degree by the repugnance for the national state and its processes of power will surely sweep up educational institutions along with other signal features of our society.*[11]

It is very difficult to be an administrator in these circumstances. Governance policies (including those of boards of trustees and university senates) are apt to be very changeable, and the circumstances that affect statesmanship will affect administration. The administrator will, for instance, have to see that his methods are "acceptable" and he will have to make everything that he does "comprehensible" to the various publics. Further, it is the traditional role of the administrator to propose solutions to the problems confronting his institution. This role will continue unchanged; however, it must be played in the new context. As Lord Morris said, "The professional administrator must research the facts and devise an operable scheme. And, by the same token, it is his task to present an acceptable scheme; for today only acceptable schemes are operable."[12]

It is clear that faculties of universities really do not want presidents and deans, that public school teachers do not want principals or superintendents, but that these administrators are necessary if the institutions are going to operate. School principals, long considered men-in-the-middle, have been deprived of virtually all authority, and now only the most astute survive through a finely tuned political acumen. The superintendency, at least in major cities, has been rendered virtually untenable. Deans do not have clearly defined jobs

because of the power of faculties, senates, or other bodies, and because of the nature of the university.

Of all the elements of society that have changed, that which has changed most significantly is the individual. Perhaps the case may be made by contrasting two statements: one by Simon in 1950 and the second by Lord Morris in 1974. In discussing the concept of authority, Simon, writing in his classic *Administrative Behavior*, said, "The superior frames and transmits decisions with the expectation that they will be accepted by the subordinate. The subordinate expects such decisions, and his conduct is determined by them."[13] Lord Morris, in speaking of the same relationship said, "The peoples do not want to be governed, and clearly they do not believe there is any real and final necessity to be governed."[14] Both statements are a bit extreme; however, each is used to describe a central tendency. Clearly, the way in which people view themselves and their relations to others in organizations has changed to the point that the theories of administrative and organizational behavior we have been using are no longer applicable. They do not describe organizational behavior, nor do they predict such behavior. They ignore the basic change in authority relationships that has been underway since World War II and that accelerated in the early 1960s.

Viewpoint of Phenomenology

In order to understand the way members view the organizations in which they work and live, it is useful to examine the philosophical position that is now very popular in Western Europe and that is gaining supporters in this country, namely, phenomenology. Greenfield raised this point at the International Intervisitation Programme when he recommended that "Research into organizational problems should consider and begin to use the phenomenological perspective."[15]

In discussing, but not defining, phenomenology, he says it is "that view which sees organizations as the perceived social reality within which individuals make decisions."[16] This leads Greenfield to define theory as "sets of meanings which people use to make sense of their world and behavior within it."[17] In short, he equates theory with common sense, and this I believe we should reject. It further brings him to a conclusion regarding theory that may well be right, but most persons will consider highly debatable, and he gets at this by quoting Mayntz:

Propositions which hold for such diverse phenomena as an army, a trade union, and a university . . . must necessarily be either trivial or so abstract as to tell hardly anything of interest about concrete reality.[18]

By espousing phenomenology Greenfield leads us to believe that using the phenomenological approach in research and preparation programs necessarily means the rejection of present administrative and organizational theories. This, however, is not a view held by all phenomenologists (as one might expect of adherents to the view that one's individual perception of the world is the most significant fact of nature). A much more mature and sophisticated view of organizational theory is offered by Denton.[19]

First, a bit of background. Denton uses some concepts that are not in the general vocabulary of the day yet are necessary to understand his point of view. Denton insists that education is "first person experience," that is, that education must be talked about in terms of the ordinary experiences of individual people, not in nomothetic terms. The concept of "steno-language" is used to refer to "definitions which are established by stipulation, by formal rule, or by ordinary usage."[20] Virtually all of the words used in organizational theory are termed steno-language by Denton and, he says, cannot be used to talk about ordinary experience. He then states a premise:

Instead of starting with a standardized language, with its cannon of exactness, which precludes all but the most limited use of terms and sentences stating idiographic, novel, open, and multi-dimensional experience, let us start with that which is given to us in ordinary experience which is sharable, verifiable, mutually observable.[21]

This leads him to the conclusion that "to talk of organization with the terms of existential language would not immediately negate, throw out, all organizational theory, but would be to question the status of such theory."[22] I note that the condition of "mutually observable" is not generally accepted by phenomenologists.

Now to get to the phenomenological critique of organizational theory. The fundamental premise is "that conceptions of knowledge and explanation, to be genuinely human, must be grounded in the feeling-states of individual persons."[23] These feeling-states cannot be expressed in terms of steno-language whose usefulness is restricted to organizational structure and process. Applying this analysis to the Parsons and Shils concept of role (the basic unit of the steno-language of organization), Denton concludes that Parsons and Shils have admitted that action must include the personality of actors, yet they exclude personality variables from their theory. Denton's basic criticism of organizational theory is stated as follows:

Given the objective nature of steno-language, one cannot construct a theory of organization, or a theory of anything, which will be consistent with the presuppositions of steno-language and, at the same time, account for, open the door to, the idiographic, simultaneous, protean characteristics of individual persons in the operation.[24]

He suggests that organization to be meaningful must be grounded in the ordinary experience of individuals and talked about in appropriate terms. He states:

All of this would suggest that to talk of organization with the existential language of our ordinary experience would be to talk of autonomous groups, spontaneous order, workers' and students' and faculties' control, and the principle of federation.[25]

There are three observations that must be made of this prescription. First, it is just that, a prescription of what the phenomenologists believe an organization should be. These concepts may or may not be of value in describing an organization. If there is students' control, then it should be used, but not otherwise. Second, are not some of these concepts couched in steno-language? I say some, because Denton does not make clear what "spontaneous order" is. It seems to me that whenever phenomenologists attempt to use existential language they end up with steno-language at a more specific level. Thus, rather than saying *personnel* (a derogatory term to a phenomenologist), they use workers, students, or faculty. Further, none of Denton's illustrations is of individuals.

Several things are clear concerning the phenomenological perspective:

1. The individual has little or no place in modern theories, nor can he, given the fact that such theories are built using steno-language.
2. Modern theories fail to get at the "real" world of the people who live and work in organizations since theories omit the "ideology" of the organization and the people in them.
3. Organizations would be quite different if they were constructed from a basis in the ordinary experience of people and discussed using existential language.

It is also quite clear that to do what Denton and, I presume, other phenomenologists ask is extremely difficult, which is at least one reason why so little organization theory has been built from the phenomenological perspective. Difficult or not, it does seem that if organization theories are to be built so as to give appropriate recognition to the people in the organization, then the phenomenological critique must be heeded.

There is another reason why new concepts must be introduced into

the study of administration and that is, many of the old concepts are proving less than useful. Two examples might suffice.

Probably the most common concept through which administrative theorists view the world is that of *role*. This concept was invented by Ralph Linton, who found it useful in describing primitive cultures. (I might say that the people he wrote about could not read, so we do not know whether they consider Linton's roles to be descriptive of their culture. Remember that our subjects can read what we say about them.) He used "role" to mean the expectations held by members for the positions in a given society. When Neal Gross attempted to use role theory in his superintendency study (the first empirical test of role theory), he found that different people held different expectations for the superintendent and the superintendent occupied many different roles both within and without the organization. He thereupon invented the concept of multiple roles.[26] Shades of Ptolemy!

I submit that the concept of role as *the* set of expectations held for a position has little value in today's complex organizations. We would be better off if we described behaviors and perceptions and dropped the role spectacles. In calling for dropping abstraction in describing the behavior of people in organizations, I believe the phenomenologist to be correct.

Much the same sort of thing has happened to the concept of *bureaucracy*. When in the Aston Studies the concept of bureaucracy was put to an empirical test it was found necessary to develop six different types: full bureaucracy, nascent full bureaucracy, workflow bureaucracy, nascent workflow bureaucracy, preworkflow bureaucracy, and personnel bureaucracy. The study indicated that the concept of a single bureaucratic type is no longer useful, since bureaucracy takes different forms in different settings.[27]

The generalization drawn from both illustrations is that theory building without constant reference to reality is a dangerous game. In fact, theories should not be built unless they are validated through research. Further, it should be noted that in both cases the trend was from a highly general concept to more specific ones. I believe this trend will continue until the concept is dropped altogether.

A NEW THEORETICAL PERSPECTIVE

It is time for a new paradigm for the study of educational administration. Modern theories are not adequate to describe or predict the behavior of people in organizations. This is because people no longer

view their organizations as they did previously, and the theories are proving to be inadequate when tested against field situations.[28] The value of a new paradigm was vividly projected in the March 1976 issue of the *Administrative Science Quarterly* in an article by Weick. He first reproduced a personal communication from James March:

> *Imagine that you're either the referee, coach, player or spectator at an unconventional soccer match: the field for the game is round; there are several goals scattered haphazardly around the circular field; people can enter and leave the game whenever they want to; they can throw balls in whenever they want; they can say "that's my goal" whenever they want to, as many times as they want to, and for as many goals as they want to; the entire game takes place on a sloped field; and the game is played as if it makes sense.*
>
> *If you now substitute in that example principals for referees, teachers for coaches, students for players, parents for spectators and schooling for soccer, you have an equally unconventional depiction of school organizations. The beauty of this depiction is that it captures a different set of realities within educational organizations than are caught when these same organizations are viewed through the tenets of bureaucratic theory.*[29]

The argument is, then, that it is necessary to view organization with different spectacles, that the theories we have been using have limited our view of what an organization is and how it operates.

There is, however, a basic question that must be discussed before moving through this section. The question is "What is theory?" and, more particularly, "What is a theory of organization?" Much of the argument among Greenfield, Crane and Walker, and myself revolves around this question, and, further, there are now definite positions being taken on the question. As I read various uses of the word *theory* today, I am more and more inclined to, but have not yet accepted, Conant's position:

> *The reader may be reminded that the words* theory *and* hypothesis *are frequently employed to describe both conceptual schemes and models, or pictures, explaining such schemes. A discussion of the definition of theory and hypothesis is often given in elementary texts, but I think such considerations of doubtful value. Because of the ambiguity it is well, perhaps, to avoid as far as possible both words in discussing the evolution of science.*[30]

It is disturbing in reading Kuhn, whom so many in educational administration consider to be the author of the definitive work in the history of science, to note the loose way in which he uses the words *theory* and *paradigm*. One diligent critic counted at least 22 different uses of paradigm.[31] Instead of being pleased at this recognition of his creativity, Kuhn discounted them as "stylistic inconsistencies" and claims to have used the term in only two ways.[32] For us, at this point, the following is of importance:

> *Scientists themselves would say they share a theory or set of theories, and I shall be glad if the term can ultimately be recaptured for this use. As currently used in philosophy of science, however, "theory" connotes a structure far more limited in nature and scope than is required here. Until the term can be freed from its current implications, it will avoid confusion to adopt another. For present purposes I suggest "disciplinary matrix": "disciplinary" because it refers to the common possession of the practitioner of a particular discipline; "matrix" because it is composed of ordered elements of various sorts, each requiring further specification.*[33]

I disagree. *Disciplinary matrix* is no improvement at all, and those who pervert the term *theory* will have a field day with *disciplinary matrix*. My view is very similar to that of Blalock:

> *Ideally, we might hope to achieve a completely deductive theoretical system in which there would be a minimal set of propositions taken as axioms, from which all other propositions could be deduced by purely mathematical or logical reasoning.* (Note the similarity to Feigel.) *More realistically we might take the model of the completely closed deductive system as an ideal which in practice can only be approximated.*[34]

I believe we should attempt to approximate the use of the word *theory* to mean a set of assumptions from which propositions can be deduced by mathematical or logical reasoning. I hope that we in educational administration reach consensus on this use and put a stop to the wide usage that has, for all practical purposes, rendered the term useless.

Positions on a New Paradigm

There are, to my mind, three major positions on the theory continuum. These range from full support for the status quo, to attempts to view organizations in a new light while maintaining a scientific approach, to the phenomenological perspective of organizations. All three have merit, but I prefer the middle position.

At one end are the supporters of the status quo. The theories and concepts we have are considered quite satisfactory and they content themselves with normal science. I believe a great deal of traditional theory has merit and should be encompassed in the new paradigm.

The middle ground is occupied by March, Cohen, and Weick, among others. It takes the view, which I fully share, that we have used a far too limited set of concepts in looking at organizations, that what we need is a new set of concepts and new theories. I shall examine some of these below.

The third position is occupied by Greenfield, Denton, and a number

of others. I have a great deal of sympathy for their position because they are highly critical of traditional theory and want to see a new approach. I have a difficult time with some of the ideas, as mentioned above in my discussion of Denton. I also wonder about Greenfield's basic contention that organizations are not "real."[35] To me, New York University is "real," the Salvation Army is "real," IBM is "real," British Airways is "real," and the Catholic Church is "real." I believe Greenfield is building a straw man when he claims that organizations are invented social reality. On the other hand, I find very congenial Greenfield's statement: "The notion that organizations depend upon meaning and purposes which individuals bring to organizations does not require that all individuals share the same meanings and purposes."[36] Especially since I wrote much the same thing twenty years ago.[37]

My major problem with Greenfield is his insistence that the central task is to develop "a theory which deals with organizations in ideological terms."[38] First, I say, go ahead and try to do it. There is far too much talk *about* theory and far too little *doing* of theory. I suggest that if he tries it, he will not have a theory at all. I suspect that he will have an ideology of organizations that may well be useful, but not theoretical.

Then there is the Crane and Walker position, which I put off the continuum. They accept all of the traditional theories, but are not particular about adhering to any standard definition of theory. Thus, while they accept the Getzels-Guba model, they also consider the writings of Mao and Marcuse to have theoretical value in educational administration. In reading Crane and Walker's article bearing the title "Theory in the Real World of the Educational Administrator," one is led to the conclusion that anything that has been useful to an administrator is *ipso facto* theoretical.[39] While, I grant, this is not an uncommon view, it should be rejected because it renders the word *theory* useless. In addition, however, they accept the phenomenological view — a logical impossiblity — but then, who ever said the world was logical?

Toward a New Paradigm

As a way of getting started, but with no intention of being definitive, I should like to present a paragraph written by Norman Drachler, former superintendent of schools in Detroit, in response to an

invitation to write about a crucial incident in his career. This paragraph was the introduction:

> *Various crises or incidents come to mind: the Detroit riot of 1967 and its impact upon the schools; the day I permitted the Black Students Club of Northeastern High to raise the Black Nationalist flag on Malcolm X's birthday, when someone stole the American flag which was to fly above it – and hundreds of war veterans appeared at the Board to protest; the months preceding decentralization when citizens recalled the four board members who had voted for an integration plan, leaving the Board without a quorum to conduct school affairs; or, during the grape boycott when the only lunchroom manager to purchase grapes was at the one high school with Spanish-speaking students.*[40]

Just how reasonable and rational are those events? What modern theory would help in their analysis and solution?

The following studies lay the groundwork, I believe, for the emerging new paradigm in educational administration.

Garbage Can Theory. The only theory developed exclusively from studies of educational institutions (eleven studies of universities) known to me bears no resemblance to any of the theories now in use in educational administration. While I would feel more comfortable with this formulation by Cohen, March, and Olsen if I could see some clear relationship between their research and the resulting theory, nonetheless the theory appears to explain certain aspects of university governance. Their theoretical framework can be summarized as follows: While most organizations can be so named some of the time, public, educational, and illegitimate ones consistently display the characteristics of organized anarchies. They operate on the basis of inconsistent and ill-defined preferences — fuzzy goals; unclear technology, that is, their own processes are not understood by their members; and fluid participation, that is, the members change frequently and devote varying amounts of time and energy to decision making and, further, the audiences and decision makers change capriciously. Decision making in such organizations is described by the authors as the "Garbage Can Model," for obvious reasons.[41]

Clark and Guba in their study of schools, colleges, and departments of education find that decision making fits the Cohen, March, and Olsen description.[42]

Environment-Situation. Taking von Bertalanffy's formulation, Emery and Trist developed a concept, "the causal texture of the environment," to account for those processes in the environment that are among the determining conditions of the exchanges between the organization and its environment.[43] Four types of causal textures were

identified following studies of change problems in hospitals, prisons, educational and political organizations: placid, randomized environment; placid, clustered environment; disturbed, reactive environment; and turbulent fields. I understand that universities would be operating in turbulent fields, since most, but certainly not all, of our troubles arise from the environment itself, not from within the organization. By a process that is not entirely clear, the authors feel that an analysis of the environment resulting in turbulent fields implies that McGregor's Theory Y is the proper course of action. This means that a new set of values must be developed — which they feel will take a generation.

Loose Coupling. Weick puts forth the concept of "loose coupling" as a way of describing organizations. He proposes that elements of an organization are often tied together loosely and that this incorporates a surprising number of disparate observations about organization, suggests novel functions, creates stubborn problems for methodologists, and generates intriguing questions for scholars.

> *The idea of loose coupling is evoked when people have a variety of situations in mind. For example, when people describe loosely coupled systems they are often referring to (1) slack times – times when there is an excessive amount of resources relative to demands; (2) occasions when any of several means will produce the same end; (3) richly connected networks in which influence is slow to spread and/or is weak while spreading; (4) a relative lack of coordination, slow coordination or coordination that is dampened as it moves through a system; (5) a relative absence of regulations; (6) planned unresponsiveness; (7) actual causal independence; (8) poor observational capabilities on the part of a viewer; (9) infrequent inspection of activities within the system; (10) decentralization; (11) delegation of discretion; (12) the absence of linkages that should be present based on some theory – for example, in educational organizations the expected feedback linkage from outcome back to inputs is often nonexistent; (13) the observation that an organization's structure is not coterminus with its activity; (14) those occasions when no matter what you do things always come out the same – for instance, despite all kinds of changes in curriculum, materials, groupings, and so forth the outcomes in an educational situation remain the same; and (15) curricula or courses in educational organizations for which there are few prerequisites – the longer the string of prerequisites, the tighter the coupling.*[44]

Informal Organization. Modern theories have attempted to deal with the attitudes and feelings of people in organizations through the concept of informal organization. The idea was that if the goals of people could not be met through the formal organizations then the members would join together to seek their goals outside the formal structure. While Denton mentions informal power structures, he states only that, "Informal arrangements will develop for the simple reason that the life forces, the spontaneity, the élan, the ordinary

experiences of persons overflow the bounds of any organizational chart."[45] Are informal structures central to organizations rather than peripheral as we have assumed? Remember that Chester Barnard developed the concept of informal organization, and he was the president of New Jersey Bell. How would he have viewed the informal organization if he had been a lineman?

Unions. Theories of administration and organization are written as though there were no unions or other formal employee groups existing within organizations. Theories of organization in education must be devised from now on to incorporate the individual as a member of two formal groups. I suppose, after talking with my union friends, that there must also be an informal power structure within the union as well as the educational institution. This means that each individual assumes at least four postures, formal and informal in the school and formal and informal in the union.

The formal decision-making structure of each school district or university must also be reconceptualized because the union replaces building faculties in the public schools and senates in the universities as the decision-making body. While there is no faculty union at New York University, for example, there is a plethora of formal groups all involved in varying ways in the formal decision process. Some of these are: Faculty Council, Deans Council, Student Senators Council, Central Administration Officers Group, President's Office, and the Administrative Management Council. There are two focal points at which they meet to develop policy: the Senate Budget Advisory Committee and the Senate. In addition, there are several unions of nonprofessional people whose policy issues are settled through collective bargaining. I have never seen a theoretical formulation that has attempted to incorporate these various elements in a university's decision-making process.

The Individual. Although the phenomenologists make a strong case for treating people in theories as individualists without the use of steno-language or categorization, they offer no examples of what this means in practice.

It is true that present efforts at theorizing in educational administration employ concepts that are stated in sociological, psychological, economic, or anthropological language, and these are abstractions. For instance, we talk of a person occupying a role. Role is a sociological abstraction. The phenomenologist would prefer to speak of the person as a basic fact. As Vandenberg puts it for education, "The task of educational theory is the restoration of the wholeness of educational phenomena as they appear within the educating perspec-

tive."[46] The ultimate task for educational administration is very much the same.

If the person is considered the basic fact, then his values, his feelings, his attitudes, what Denton calls his "soul" would be reproduced in some way. But would we then have a theory? Remember that theories are important only to the extent they give rise to the activities that Kuhn calls "normal science," that is, the testing of hypotheses derived from the theories. If the individual is the basic fact, how is he incorporated into theoretical statements unless stenolanguage and categorizations are used?

It may very well be that we should be reminded that theory-building is not all of administration. Practice goes far beyond the realm of theory. It could be that the hopes of the phenomenologist can be achieved in practice, but not in theory.

Rather than attempting to build theories in which individuals are central, we must try to construct theories that reflect the actual balance among people, organization, and environment so as to be of value in specific situations. It seems to me that the theories employed in educational administration are essentially Great Man theories, that is, it is assumed that if the administrator is capable enough, he will be able to comprehend and resolve all situations in a satisfactory manner. It seems to me that a number of recent studies would lead to precisely the opposite conclusion, namely, there are situations that no administrator, however capable, can comprehend and resolve unless changes are brought about to alter the environment or the organization. I have in mind the Nicholas, et al., study of the climates of four elementary schools and the differences in administrative behavior found in each.[47] In addition, there are now several studies that attempt to build classification systems of climates and environments.

An experimental study by Frederiksen, et al., employed simulation to test the effect of climate on the people working in it.[48] The major finding was that the mean Productivity score, that is, the amount of work completed, is significantly affected by the consistency of climate conditions. More important are the four climates that were contrived and the fact that it was clearly and significantly demonstrated that the climates made a difference in administrative performance.

Clark and Guba conclude that, "The culture of institutions of higher education is predominately idiographic (i.e., emphasizing the self-actualization of the professor) rather than nomothetic (i.e., emphasizing the goals of the institution)."[49] Administration in such an organization must surely demand a different theory than IBM or the Army, which are far more nomothetic than the university.

If a theory is built on observations of organizations — one by one — it seems to me that it is apparent that individuals (except for the President or certain mavericks) form groups to attain their personal goals and often the organizational goals. Thus public school teachers have developed strong unions and associations designed to achieve the goals of individuals. Even universities in which individuals have traditionally worked on their own are moving toward unionization or other types of group action. The individual as a member of a group of his own choosing is a fact of organizational life.

Summary

What, then, are some of the dominant ideas that might well be influential in the development of the new paradigm?

1. The end of the *idée fixe* in modern life. Just as in modern art the artist pictures life in nonrepresentational ways, so too will the social theorist recognize that his work should produce theories that encompass the essence of modern life.
2. Theories will have to develop new approaches to the concept of authority and acknowledge the fact that people do not want to be governed.
3. The key administrative process in organization is very likely to be *bargaining* and not necessarily collective bargaining. It could very well be that the *Barnard-Simon theory*, which is essentially a bargaining theory, will be prominent in the new paradigm.
4. New theories are likely to be highly specific and focused on particular types of organization. The search for a general theory of administration will be long delayed.
5. There will be numerous efforts to build theories using ideology and values for axioms, with propositions predicting behavior dependent on the axioms. It is dubious that these efforts will succeed.
6. Emerging theories will likely use situations and situational variables as axioms.
7. Methodologically, emerging theories will be far more complex than present theories.
8. New concepts entirely different from ones with which we are familiar will be used (for example, loose coupling).

WHAT NEXT?

When the theory movement in educational administration got underway in the 1950s the big drive was to develop the "theory." Possibly

in an effort to overcome the nontheoretical, anti-intellectualism of the times, several of us attempted to construct highly abstract and formally elaborate theories. While it was good fun at the time it was not particularly helpful and, in fact, might even have been harmful because people were led to believe we were much farther along than we actually were. I hope that the effect of this paper will not be to stimulate another similar round of theory production. Rather, we should pay attention to Trow's review of *Administrative Theory in Education*, a volume of papers edited by Andrew Halpin, which contained most of the theories that were to be used widely.[50] Trow said:

> *What is conspicuously absent from most of these papers is any evidence of theoretically guided research in educational administration actually being done; substantive problems being defined and empirically studied; uniformities being discovered, related to each other, and accounted for; or any other form of theorizing actually going on in and through research.*[51]

It is never too late to take good advice.

NOTES

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Intellectual turmoil in educational administration

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The title of this paper may be an exaggeration. But if educational administration is not in a state of intellectual turmoil, it should be, because its parent, the field of organizational theory, certainly is.

I believe that Willower expressed the consensus of scholars in educational administration when he argued that theories now employed have resulted in much that is useful and that a great deal of research on these theories remains to be done.[1] I, too, would not deprecate the theoretical work of the past. Without that work there would not be any discussion of theory today. Yet, it is clear to me that virtually all of the research being done in educational administration can be characterized by what Kuhn calls "normal science," that is, research on problems derived from the now-accepted paradigm. Further, educational administration is particularly susceptible to Herda's generalization: "Criticism and the 'growth of knowledge' do not characterize the social sciences."[2] She also observed, in discussing educational administration: "Analysis and synthesis of studies and findings are lacking. Critical discussions are virtually nonexistent among the various writers advocating a need for theory in educational administration."[3] In fact, one might say that theorists in educational administration hold a view similar to that of Noel Coward who is reputed to have said, "I love criticism just so long as it's unqualified praise."

There are some signs that there is an awakening of criticism in educational administration.[4] The level and intensity of that criticism is, however, nowhere near that in the parent field of organizational theory. The central purpose of this paper is to present the major criticisms of organizational theory that pertain to educational administration as those critiques exist both in the literature and in unpublished seminar papers. I will comment on the criticisms and draw some implications for research and theory building in educational administration.

I approach this task with a certain sadness, because it is not possible to do the job without challenging virtually all of the premises that I have accepted during my career. I feel much as Perrow did when he wrote,

> *Since I am still very much a mainstream theorist, the reader should bear with me in the quite painful process of trying to think oneself out of a paradigm one has lived with, even contributed to. It is difficult to think that one's work on, say, goals or technology, while not wrong in any normal sense, is largely irrelevant in the face of notions that were dimly recognized but put aside.*[5]

Perrow's phrase "think oneself out of" is central to this paper; as a result, the paper will be awkward at times and the conclusions will be groping, not polished and positive. I have long had an uneasiness about the assumptions underlying organizational theory. While it may be only a type of face-saving for me, Perrow's comment that "notions that were dimly recognized but put aside" is apt. My book *Administrative Theory* should have been revised years ago, but I did not feel comfortable in doing so. I think that this paper justifies that discomfort.

CRITICISMS OF ORGANIZATIONAL THEORY

The criticisms of organizational theory range from the petty to the profound. I shall not dwell on the former. Both Perrow and Greenfield, for instance, seem to think that Barnard's theory of organizations as cooperative systems was somehow discounted because he once swore at a group of workers.[6] Then there are the rhetorical flashes, such as Clegg and Dunkerly's accusing the *Administrative Science Quarterly* of "hegemony" in traditional research methodology.[7] I shall present brief descriptions of substantive criticisms with comments on each and follow with a discussion of methodological and philosophical issues. The paper ends with an attempt to suggest directions for the future of theory and research in educational administration.

Demystifying Organizations

Perrow puts well what is on the minds of many critics of organizational theory in his paper "Demystifying Organizations."[8] Perrow states, "The reality being asserted in mainstream theory is that organizations are, or can be, rational instruments of announced goals,"[9] and "Deviations from these goals are due to such things as poor motivation, faulty communications, misperceived goals, poor coordination, inadequate resources, poor management or faulty structures, according to most theories."[10] Perrow then says that an alternative position is worth

exploring, namely, that "announced goals are one of the least important constraints on organizational behavior. . . . Instead, organizations are resources for a variety of group interests, and the announced purposes, while they must be met to some limited degree in most cases, largely serve as legitimating devices for these interests, or a mystification of reality."[11] And, finally,

> *There is nothing novel in the view that organizations serve many functions; what may be novel is to take this view so seriously as to put it at the center of our theory, rather than leave it as a peripheral routine observation or qualifier while we vigorously explore the impact of technology on structure or leadership on productivity. . . . To complete the demystifying process means to present an alternative definition of organizations and explain how well-meaning people and institutions participate in the fiction of intended rationality in the service of official goals.*[12]

There is, in fact, a group of critics who talk about demystifying organizations, that is, putting the concept of organizations as rational instruments of announced goals at the periphery of theory and replacing it with the view that organizations serve many functions.[13] When goals are placed at the periphery and the environment and organizational participants are placed at the center, a view quite different from the goal-centered concept of organizations emerges.

Perrow argues that human-service organizations should be judged on how well they accomplish their regulatory functions, not on how well they achieve their announced goals. A second function of human-service organizations is to absorb a part of the work force of the nation; and a third function is to provide resources for other organizations.

Regarding the internal structure of human-service organizations, Perrow argues that executives are judged by the size of the organization's budget, the executive's contacts with elites, the accommodations the organization has with other organizations, and the number of programs it has. Since these factors are easy to measure, while effectiveness is very difficult to gauge, the executive acts accordingly. Turning to the workers, Perrow contends that there is sufficient evidence to conclude that neither the goals of the organization nor efficiency is what is on the workers' minds. He suggests that the workers are concerned about the retirement system, being able to pick up office supplies or food from the kitchen or furniture for their own use, a recreation center, being able to exercise some control over their own work, and being sure of having a job as long as they want one, among other things. Putting this argument in the context of higher education: professors use university offices, computers, and the like for their personal profits; they have secretaries type manuscripts; they use university stationery for consulting memoranda; and they insist on job security through tenure.

The point to keep in mind is that according to traditional organizational theory workers *must* accept the goals; if they do not, there is a pathology that must be corrected. But it is not pathological, Perrow contends, it is fundamentally characteristic that workers, including professors, use organizations for their own ends just as managers, executives, and other groups do. Perrow includes organizational theorists in this generalization, saying, "We are just another group trying to use a multi-use collectivity, getting paid to do so, and building our careers on it."[14]

The demystification argument is quite convincing. It has been my experience both as an educational administrator and as a theorist that goals are quite important to top executives and board members but of little consequence to teachers or professors. Yet, organizational theories have been based upon the perspective of top executives, with only a modicum of attention paid to the interests of lower participants. This dichotomy has been expressed through the use of the concepts of "formal" and "informal" organization. Formal organizational theory has taken goals as provided by top executives; the theory of informal organization has built upon the ways in which other organizational participants have sought to achieve their own goals, which are generally not the goals of top management. While traditional organizational theory has taken the executives' statement of goals as central, and Perrow wonders about taking the workers' and other groups' goals as central, both approaches seem wrong to me. Rather, there should be a way of describing organizations that tries to determine, not a priori but in actuality, what the particular organization is attempting to do, and, in particular, what the people in it are attempting to do.

Women, Race, and Unions

In this section, I note a number of criticisms which appear to be obvious and uncomplicated. One wonders why more attention has not been paid to the disregard for the plight of women and minorities by organizational theorists and to the existence of unions.

It is plain to an observer of organizations that women are remarkably underrepresented in the upper levels of organizations of all sorts, at least in the United States, Canada, and Western Europe. One of the few pieces of systematic research on this subject was done by Wolff, who studied women at work and women in trade unions in Britain and attempted to use organizational theory to explain her findings.[15] She concluded that women are at a disadvantage in relation to men in most organizations; that traditional organizational theory can account for little that is found

in practice; and that organizational theory must be supplemented by a sociological grasp of extraorganizational influences on organizational practice in order to understand what is happening to women in organizations. Her paper argues for theory that encompasses far more of the sociocultural environment in which organizations exist.

While I have not found a study that does for the various races what Wolff has done for women, it would seem that organizational theory is unable to account for the prevailing distribution of races in organizations.

Theories of organizations have been written as though there are no unions.[16] While it is true that only about 20 percent of the work force of the United States is unionized, unions exist in major industries and have a substantial impact on all workers, unionized and nonunionized. In addition, unions have become commonplace in the public schools and have made inroads into higher education.[17] Failure to incorporate unions into organizational theories has caused the theories to be incomplete in that a major influence on the behavior of both managers and workers has been omitted.

The prevalent practice in research and theory in educational administration is to use theories and models developed in the broad field of the social sciences for the study of educational organizations. This practice has been questioned on a variety of grounds and for opposing reasons.[18] One of the cogent reasons advanced is that universities are quite different, in functional ways, from factories, corporations, governmental agencies, the military, etc. The analysis of precisely what the differences are, however, has been lacking.

Efforts to unionize major universities and colleges have generated an analysis of the professoriate in relation to the management of universities that is contrary to all known organizational theories. In an effort to overthrow the results of a collective bargaining election, Yeshiva University argued that professors are not employees under Section 2(11) of the National Labor Relations Act.[19] The United States Court of Appeals of the Second Circuit ruled that the full-time faculty at Yeshiva has management functions because it "effectively recommends" the hiring, promotions, salary, and tenure of faculty.[20] Further, the faculty has "extensive control" over standards of admission, the curriculum, the grading system, and the graduation requirements. The court said, "When faculty members have such power they are no longer simply exercising individual professional expertise. They are, in effect, substantially and pervasively operating the enterprise." The National Labor Relations Board (NLRB), the defendant in the suit, contended that the full-time faculty acts "on its own behalf and not that of the University." But the court found that the faculty "has initiated, and the administration has repeatedly accepted,

major policy determinations which constitute the essence of the University's venture." The court concluded: "The faculty is the school."

Even if the Supreme Court overturns the finding of the lower court, the implications for organizational theory are clear. Theories applying to organizations such as factories cannot automatically be assumed to be descriptive of universities. If the Supreme Court upholds the Court of Appeals in its Yeshiva decision, the value of traditional organizational theories to understanding universities will be nil. As Bunzel points out in *The Wall Street Journal* of September 29, 1978: "Part of the genius of most first-rank universities is that no individual or group has full control over it. The line between management and employee is an 'alien concept' because the faculty actively participates with the administration and the governing board in looking after the best interests of the campus." In fact, the very meaning of the words *employee, administration,* and *governing board* all need redefinition. They have no meaning in first-rate universities and colleges if they are defined in reference to traditional organizational theory.

External Control of Organizations

A common theme running through much of the criticism of organizational theory is that theorists pay too little attention to the effect of the environment on the organization. Those who make this criticism recognize that most organizational theorists admit that organizations have environments and that these environments have an effect on organizational behavior, but these critics contend that the environment is the most powerful factor affecting organizational behavior. There are two approaches to this argument: the resource dependence perspective[21] and the concept of networks.[22]

The main argument of the resource dependence perspective is that organizations are controlled by environmental contingencies, or, putting it differently, by dependence upon the environment for strategic resources.[23] The contention is that because organizations are externally controlled, theories based upon internal control are of limited utility and that the behavior of an organization can be predicted more accurately from a knowledge of resource dependencies than from a knowledge of management strategies.[24]

Pfeffer and Salancik approach resource dependence by discussing how organizations survive. They survive by being effective, that is, by managing the demands of the interest groups on which the organizations depend for resources and support. There are many ways of managing demands, including giving in to them.[25] "The key to organizational

survival is the ability to acquire and maintain resources."[26] "Problems arise not merely because organizations are dependent on their environment, but because their environment is not dependable."[27] The thrust of the Pfeffer and Salancik position is that organizational survival and success are not always achieved by making internal adjustments because those adjustments can be overcome by contextual variables.

In developing their argument, Pfeffer and Salancik redefine the concepts of efficiency and effectiveness. Organizational effectiveness is an *external* standard of how well an organization is meeting the demands of the various groups and organizations with which it interacts. On the other hand, efficiency is an *internal* standard of how well something is being done. The difference between the two, and how Pfeffer and Salancik view the importance of each, is illustrated by the story of Governor Ronald Reagan's curtailing the amount of money going to the California State University system. As they put it:

> *He was concerned that state university campuses, particularly Berkeley, were indoctrinating students in radical, left-wing ideas. In response to these political pressures and to forestall further budget cuts, the administrators attempted to demonstrate that they were educating students at an ever lower cost per student. Not surprisingly, this argument had little impact on the governor, indeed it missed the point of his criticism. Producing revolutionaries at a lower cost was not what the governor wanted; rather he questioned whether the universities produced anything that justified giving them state funds.*[28]

A third concept Pfeffer and Salancik used in the resource dependence perspective is that of constraint, described as whenever one response in a given situation is more probable than any other response to the situation regardless of who is acting.[29] "The concept of restraint explains why individuals account for relatively little variance in the performance and activities of organizational systems."[30]

The resource dependence perspective gives rise to an interesting way of conceptualizing the manager, who is seen as a symbol of the organization and its action. As is the practice in many fields such as sports, industry, and, to some extent, government, the manager is the person to be fired when things go wrong, because this act gives the governing board or president a feeling of control over organizational matters. (It is surprising, though, that on occasion changing managers does result in the organization doing better.) The other major function of management is to process external demands so that the organization can gain and hold the resources it needs to survive.[31]

The resource dependence perspective leads to an interesting view of change. It is certainly clear that changes in the environment bring about changes in organizations. For example, with the decrease in demand for teachers and other educational personnel, many schools of education

have changed their missions and, indeed, their names in an effort to survive.[32] This has lead to the idea that one way to change an organization intentionally would be to change its environment.[33] Efforts at lobbying in Washington or in state capitals to gain resources that change the environment would be one such example.

The resource dependence criticism is particularly valid for theories in educational administration. While my theory of change, for instance, is externally centered, my theory of administration as decision making is almost completely internal.[34] The most successful theory in educational administration, Getzels-Guba's, illustrates the resource dependence criticism to the word. Following a diagram in which the "environment" is drawn as the boundary, Getzels writes, "Although the *general* model includes the five dimensions, which must be borne in mind when considering any element of social behavior, the basic *operational* model in the present analysis of administrative processes in the educational context *is composed* of these salient factors: the interaction of *role* and *personality* in the context of *value*."[35]

Resource dependence offers an alternative that should be considered. Pfeffer and Salancik do demonstrate considerable support for their approach. As Benson points out, "Much of the book [*The External Control of Organizations* by Pfeffer and Salancik] consists of empirical tests of specific propositions generated from the resource dependence perspective. . . . Separately, each study provides modest support for the tested hypotheses and for the overall perspective. Compiled in one volume, the studies provide added support and lend a high degree of plausibility to the resource dependence view."[36] While the approach is plausible, there is little evidence to support the contention that the external environment should replace internal organizational concerns at the center of organization theories.

Benson raises a fundamental problem for resource dependence.[37] Organizations' responses to external control are complicated by the relevant environments they construct for themselves. These environments are determined by differential perceptions and power of organizational components. Teachers, for instance, see the world in ways that differ from school board members or custodians, and each group, in turn, has differential power within the organization. "Thus," says Benson, "responses to the environment are not simple, direct relations of a totally rational organization to an objective set of resource dependencies."[38] Pfeffer and Salancik deal with this problem in a rather traditional way by invoking open systems theory, which would lead to a prediction that the organization would tend to establish an equilibrium with its environment.

An interesting test of the resource dependence theory might be constructed by comparing public and private universities. While most public universities receive some funds from private sources, and virtually all private universities receive financial support from federal, state, and local sources, universities at the extremes — that is, those that are most public and those that are most private — could be compared to determine if there are differences between them in goals, organizational structure and functioning, and in the ways in which participants view their organizations and behave in them.

Also prominent in the approaches to conceptualizing the external control of organizations is network theory, in which there is the contention that case studies of single organizations do not provide an adequate understanding of complex social structure in modern communities.[39] Perrow goes even further when he concludes, after an analysis of the work of Warren, Rose, and Bergunder: "As they repeatedly insist, focusing upon one organization, or a pair of organizations, would be quite misleading; only when the network is examined is the single organization placed in the proper perspective."[40]

It is being contended that in order to understand what is happening in one organization, it must be studied in relation to the organizations with which it interacts. For instance, the study of a single school without a study of its relationship to other schools or elements of the school system would conceal a great deal of what goes on in the school. The concept of coupling has been developed to describe the relations of units in systems.[41] Units can be either loosely or tightly coupled. Without exploring network theory more deeply, it is safe to say that the point is made in the literature that it is more useful to study a unit in relation to other units than in isolation.

If the only contention of the network theorists is that a single organization cannot be adequately understood unless it is studied in the context of its network, there can be little debate. If, however, the contention is that the network must be placed at the center of organization theories, then it must be said that the point has not been made in the literature. It seems that network theory will find its proper place as one of a number of commentaries on the inadequacies of traditional organizational theory.

Universality of Organizational Theories

Virtually everyone who has written on theory in educational administration has assumed that theories developed in one type of organization are useful in all other types. However, the idea that theories have universal applicability has come under scrutiny and increasing criticism. In

particular, there is doubt that organizational theories are applicable automatically to higher education.

The first major statement on this issue is by Millet, who published *The Academic Community: An Essay on Organizations* in 1962, in which he developed this thesis:

> *I believe ideas drawn from business and public administration have only a very limited applicability to colleges and universities. More than this, the essential ideas about business and public administration, such as they are, may actually promote a widespread and unfortunate misunderstanding of the nature of the college and university in our society.*[42]

While Millet's thesis received little support — in fact, the literature of administration developed in the opposite direction — the occasional studies done in higher education tend to verify his argument. Hill and French, in a study on "Perceptions of the Power of Department Chairmen by Professors," concluded: "The findings reported here tend to confirm the impression of a number of students that colleges are unique kinds of organizations."[43] In the only comprehensive bibliographical analysis that I could find, Olive offers the hypothesis that existing theories of organizational behavior and administration may not be applicable to present-day universities. In support of her hypothesis, she says:

> *Governmental agencies and business corporations have an internal organization which is unlike that of the university. Among the differences mentioned are these: the goals of the university are not specific and clearly defined; the product or service produced is not tangible; the customer (student) exerts limited influence; the employees (faculty) are dedicated to their specialized fields, not to the employing institutions; and the decision-making process is diffused in a way not typical of other forms of organization. Such differences suggest the inadequacy of present theories on problems of university administration and the incontestable and urgent need for new thinking and new approaches to research in this field.*[44]

There are few empirical tests of the relevance of theoretical work in one field to another field. One such study, which did attempt to use a theoretical frame developed in industry and government to categorize New York City school personnel, found significant differences.[45] These were summarized as follows:

> *A significant distinction between Presthus'[46] and our studies, however, occurs in the fact that some two-thirds of all teachers were found to be either pupil-oriented or intellectuals. Presthus has no categories even remotely resembling these, leading us to question the wholesale and indiscriminate application of studies of business, industry, the military, and the federal government to education.*[47]

Blau's study, *The Organization of Academic Work*, offers the opposite conclusion. He attempted to determine whether academic institutions and government bureaus and private firms were homologous, that is, exhibited empirical regularities in size, differentiation of structure, and

administrative structure. He concluded: "The administrative structure of academic institutions exhibits a remarkable homology with that of other institutions."[48] It may be that if one compares universities and colleges with other organizations on purely structural grounds, without studying how the structures work, they will appear to be similar. Further, the use of a particular model, in this case bureaucracy, biases the results of the research.

The only organizational theory developed exclusively from studies of educational institutions (eleven studies of universities) is by Cohen, March, and Olsen,[49] and it bears no resemblance to any of the theories now in use in educational administration. I would feel more comfortable with the Cohen, March, and Olsen formulation if I could see some clear relationship between their research and the resulting theory; nonetheless, their theoretical framework explains certain aspects of university governance. It can be summarized as follows: While most organizations can be so named some of the time, public, educational, and illegitimate ones consistently display the characteristics of organized anarchies. They operate on the basis of inconsistent and ill-defined preferences — fuzzy goals, if you will; unclear technology, that is, their own processes are not understood by their members; and fluid participation, that is, the members change frequently and devote varying amounts of time and energy to decision making, and, further, the audiences and decision makers change capriciously. Decision making in such organizations is described by the authors as the "Garbage Can Model," for obvious reasons.

There is no clear evidence to support the contention that organizational theories can automatically be considered to be general or universal. Further, the theorist in higher education administration must give very serious thought to the idea that colleges and universities have characteristics that set them apart from other organizations and, as I shall discuss later, that it is probable that higher education is not sufficiently homogeneous to constitute a set for theoretical purposes.

Marxist Critique

An increasingly popular theme of critical analysis of organizational theory is based on a Marxist view. This Marxist critique includes a number of variations called neo-Marxism, ethno-Marxism, and the Marxist dialectic. The Marxist view interprets organization life in terms of power, conflict, contradictions, crises, dialectics, and class, and this type of interpretation is the basic thrust of the Marxist critique of traditional organizational theory.

Clegg, for instance, contends that "Much of . . . organizational theory has been developed at the interface of capitalist theory and capitalist practice in the academic institutions of business."[50] Heydebrand argues that "Organizational theory, like other theories in the social sciences, has been dominated by powerful ideological forces which, taken together, have more or less successfully reproduced and legitimized the structure of capitalist society."[51] In contrast, Perrow observes that "the ethno-Marxist approach challenges conventional organization theory on almost every score."[52] Perrow uses the prefix "ethno" to catch the flavor of ethnography, ethnomethodology, phenomenology, and symbolic interaction that characterize many of the Marxist approaches. Probably the article that most clearly supports Perrow's contention is one by Brown entitled "Bureaucracy as Praxis: Toward a Political Phenomenology of Formal Organizations."[53] However, the Marxian dialectical approach is presented most clearly by Benson, who sees the organization as a concrete, multileveled phenomenon beset by contradictions that continually undermine its existing features.[54] He points out that a dialectical view is fundamentally committed to a concept of process in which theoretical attention is focused on the transformation through which one set of arrangements gives way to another: "There are four principles of dialectical analysis — social construction/production, totality, contradiction, praxis."[55] The central idea of social construction/production is that people are continually constructing the social world, which is Greenfield's major thesis.[56] The concept of totality means that any particular structure must always be seen as part of a larger, concrete whole, not as an isolated, abstract phenomenon.

Contradiction is seen by some writers, such as Heydebrand, as the central principle of the Marxist critique. What it means is that the process of social construction contains contradictions. Benson's example is "an integral part of capitalist social formations is that they are antithetical to the interests of labor, yet the functioning system maintains or reproduces the contradiction."[57] Heydebrand illustrates the concept of contradiction with a study of the organizational contradictions within judicial systems. In an effort to introduce efficiency and to increase productivity, both administrative and judiciary reforms have been introduced. This has meant, on the one hand, the introduction of administrators into the courts and, on the other hand, more judges and court personnel. These two developments tend to contradict each other with the result that "adjudication and the adversary process are increasingly replaced by negotiation, arbitration, and ultimately administration."[58] He concludes, "The universalism of cost-effectiveness, under the ideology of scarce resources, comes into contradiction with the universalism of formal rationality and equality before the law. Courts maintain,

reproduce, and legitimize the structure of capitalist society, and thus come into contradiction with themselves as that society continues to change."[39]

The fourth principle is praxis, meaning action or activity or practice. Benson defines praxis as "the free and creative reconstruction of social arrangements on the basis of a reasoned analysis of both the limits and the potentials of present social forms."[40] What this means is that theories, for instance, reflect the social context in which they are created and the practical concerns of their creators. Theories also are deeply involved in the creation of organizations, and they guide actors in their organizational behavior. From this tautology Benson concludes: "There is, then, a dialectical relation between organizational arrangements and organizational theories."[41]

The Marxist critique is in its beginning stages, and its advocates, while enthusiastic, ask only that far more research be undertaken. It stands in complete opposition to traditional organizational theory, and the contrast is so distinct as to challenge conventional theorists on every point. It is a blatant effort to develop theories on an ideological basis, but then Marxists and others contend that traditional organization theory itself has an ideological basis, albeit a covert one.

There are obvious problems within the Marxist critique. It is, for example, full of inconsistency. The first principle, "social construction/production," that people create their own world, is contradicted by the concept of totality, which indicates *how* people should construct their world. Then there is Heydebrand's contention that ethno-Marxist theory is applicable to all organizations. This seems to be an example of the overenthusiasm that characterizes the traditional theorists, and Heydebrand is probably in error too.

Ethno-Marxism is interesting because it raises questions concerning traditional theory which must be rethought. The Marxist finds in every aspect of organization a meaning significantly different from that seen by the organizational theorist working in the traditional route. For the Marxist, rules are not so much ways to achieving the stated goals of an organization as they are means to controlling people and exercising power; the hierarchy does not so much represent levels of ability as it does the maintenance of class structure. These are but two examples of how the schools of thought differ. It will be very interesting to see in what form organizational theory will emerge from the Marxist challenge.

The Attack on Positivism

It is clear that organizational theory as it is known today was developed largely, if not entirely, through the assumptions and methods of logical positivism. This philosophical approach is now under heavy attack from many sources. One of the most vigorous assaults on positivist science is that of Susman and Evered, who launch their attack thusly:

> *What appears at first to be a crisis of relevancy or usefulness of organizational science is, we feel, really a crisis of epistemology. This crisis has arisen, in our judgment, because organizational researchers have taken the positivist model of science which has had great heuristic value for the physical and biological sciences and some fields of the social sciences, and have adopted it as the ultimate model of what is best for organizational science. By limiting its methods to what it claims is value-free, logical and empirical, the positivist model of science when applied to organizations produces a knowledge that may only inadvertently serve and sometimes undermine the values of organization members.*[42]

Those who attack positivism invariably offer alternatives that include action research[43] and theory of practice,[44] two vague and ambiguous concepts that are apparently used for their rhetorical rather than their scientific value. Others admit, as does Greenfield, that the result of the rejection of positivism and the substitution of what appears to me to be the assumptions of ethno-Marxism is ". . . not a new theory of organization, but a different way of looking at organizations."[45] He goes on to say, "Within these assumptions, there probably cannot be *a theory* of organizations."[46]

There are two ways of viewing Greenfield's conclusion. The first applies to Susman and Evered and to Culbertson, as well as to Greenfield, and it is that action research, theory of practice, and ethno-Marxism all offer interesting and often useful information about organizations. These, and similar approaches, also offer individuals an interpretation of their own experiences. But what is learned in these ways is not scientific knowledge, it is not generalizable, and often it is of little value to anyone other than the individual using the approach.

The second way of interpreting Greenfield's argument is to conclude that there cannot be scientific theory concerning organizations. This might well be what Greenfield means, since he is familiar with the philosophy of Wittgenstein, whose work might be so interpreted. While reference to Wittgenstein in the literature of organizations is rare, it appears that his work and ideas influence those who attack positivism. Wittgenstein was a logical positivist during his early years, then broke with the positivists, and is responsible for two books which greatly influence modern philosophers.[47] The books are in the form of series of

aphorisms, the *Tractatus* being Wittgenstein's doctoral thesis at Cambridge and *Philosophical Investigations* having been assembled from notes found after his death. His methodology is that of logical grammar, and his ideas have generated tremendous interest among philosophers. Many scholars make the interpretation of Wittgenstein their life work.

While Wittgenstein, of course, never discussed organizational theory, nor do his philosophical disciples, he does speak to our general concerns. To quote part of proposition 6.53 from the *Tractatus:* "The correct method in philosophy would really be the following: to say nothing except what can be said, i.e., propositions of natural science. . . ." Wittgenstein's biographers elaborate on this to say, "There simply cannot be any meaningful propositions except those of natural science, . . ."[68] which might be interpreted to mean that a physical science-type theory of organizations is impossible.

The aphorism concerning propositions of natural science was enlarged upon by Wittgenstein in *Philosophical Investigations.* He gave as an example the concept "games" as typical of a set of concepts that could not be rigorously defined. He opened his discussion by raising the question, "What is a game?" He then asked us to look at all sorts of games and consider what is common to them:

> *Don't say: 'There must be something common, or they would not be called "games"' — but look and see whether there is anything common to all. — For if you look at them you will not see something that is common to all, but similarities, relationships, and a whole series of them at that."*

This analysis was picked up by Weitz, first in an article entitled "The Role of Theory in Aesthetics," and later in a book entitled *The Opening Mind.*[70] Weitz reasoned that "game" was an *open concept,* that is, "Its conditions of application are emendable and corrigible; i.e., if a situation or case can be imagined or secured which would call for some sort of *decision* on our part to extend the use of the concept to cover this, or to close the concept and invent a new one to deal with the new case and its new property."[71] He contrasts the open concept with the *closed concept* in this way: "If necessary and sufficient conditions for the application of a concept can be stated, the concept is a closed one. But this can happen only in logic or mathematics where concepts are constructed and completely defined. It cannot occur with empirically descriptive and normative concepts unless we arbitrarily close them by stipulating the ranges of their use."[72] Whereas *game* is an open concept, a particular game can be a closed concept. Weitz illustrates this in a homey way by saying, "'Major-league baseball,' . . . unlike 'game' though fluid and flexible enough in its history, can perform its assigned role under the overall condition that it is governed by a set of definitive criteria so fixed as to ensure that no unresolvable doubts and disputes can arise."[73]

It seems that we can move the argument of whether or not there can be natural science-type theories of organization out of the realm of speculation and prejudice into the logical grammar of Wittgenstein and Weitz. Several questions should be asked: Is the concept "organization" of the same order as "game" in that both function under a disjunctive set of criteria that corresponds to a certain family of resemblances, but these criteria are neither necessary nor sufficient? And, contrariwise, is it clear that "organization" is not a closed concept? Is there a set of necessary and sufficient criteria for the definition of the concept of organization? Are there, however, specific sets of organizations that might meet the definition of a closed concept (analogous to the "major-league baseball" of Weitz)? What might these criteria be? It certainly seems that the logical grammar of Wittgenstein and his followers is a useful area to explore in an effort to resolve the epistemological question of the nature of theory of organizations.

I should point out, however, that Wittgenstein offers no solace to those who wish to introduce their private selves into theoretical formulations. Janik and Toulmin write:

> *For might one not have urged that, on his own later principles, the very intelligibility of words like* **good** *and* **right** *is as dependent as that of all other linguistic expressions on the acceptance of those shared language games and forms of life within which they are given their standard uses, and by reference to which alone we can understand one another's choices, decisions and scruples? Surely, his own later position implies that the concept of "values" itself relies for its meaning on the existence of certain standard and recognizable modes of "evaluative" behavior? To this extent Wittgenstein's later philosophy of language could neither justify nor refute, in principle, any complete dissociation of the realm of values from the realm of facts. . . . All that is certain is that, whatever the strict implications of his later position, the absolute dichotomy of facts and values was of great importance to him — of greater importance, indeed, than any particular philosophical argument that might have been put forward to underpin or justify it.*[74]

Weitz, in speaking of Wittgenstein's position on private mental states, is emphatic: "His logical behaviorism denies *in toto* one kind of concept, the concept whose necessary and sufficient criteria are privately mental; it denies as well one kind of necessary condition, the criterion of a private state."[75]

NEXT STEPS

While I have been lengthy, I have been neither complete nor detailed in my cataloging of criticisms of organizational theory. Leadership theory is in a state of complete disarray.[76] I have, in another paper, discussed

the abstract nature of organization theory and its remoteness from the practitioner."[7] And I am sure that there are other criticisms, but enough is enough. At this point I turn to the steps that should be taken in the face of the criticisms discussed above.

1. I believe the most important first step to be taken in the evaluation of current organizational theory is to clarify and come to some agreement on the epistemological question: Can there be a science of organizations comparable to the science of physics or chemistry? We must have answers to the questions: What kind of concept is *organization?* Is organization a closed concept to which the methods of natural science are applicable, or is it an open concept, such as *art,* in which case quite a different approach is necessary. If organization were an open concept, aesthetics would be a more appropriate model for building theory than physics.

The analysis of concepts should not be limited to *organization,* but also to *employee, administration, governing board,* and all the other concepts that we have used without sufficient contextual definition. Resolution of the epistemological issue is, then, the first order of business.

2. However, even as the epistemological question is settled it seems clear that efforts to develop a general theory of organizations should be replaced by efforts to develop much more restricted theories. Organizations differ so markedly that we should not only be interested in theories about educational organizations, but in theorizing about particular types of educational organizations. How far we should go is problematic, but surely ivy league universities differ markedly from California junior colleges, which differ, in turn, from the New York City public schools. Efforts to develop organizational theories that are distinctively educational should be given considerable support.

3. The critics are vigorous, but they are often sadly lacking in evidence. There is little or no empirical support for the bulk of the criticisms, which at the present time have only face validity. Further, few of the critics define organization theory or identify the theory at which their criticism is directed. They seem to believe that organization theory is homogenous and monolithic, when in fact there are distinguishable and significant differences among the theories, as is evidenced by the study *Developing Taxonomies of Organizational Behavior in Education.*[9] While I agree with Gibson [9] who asks that we stop collecting data and give our time to rethinking our conceptual bases, I hope we would not forget what it is we are doing in our theorizing.

4. The critics of organization theory are persuasive, but they should not be allowed to convince us that everything we have done to date should be thrown out. What is needed is a careful sifting of research and

theoretical findings to determine what is useful to what kind of organization and what type of organization member and in what context. In other words, just what in the research and theory on organizations is useful to us in education?

Educational administration is not the only field in which people are concerned with transfer of knowledge. Lichtman and Hunt reviewed a large number of theories of organization and concluded that "the extreme variability found within and among organizations renders one-sided normative theories less useful in understanding organizational behavior than models that recognize situational contingencies."[80] Whether one can safely borrow research or theory done in a particular type of organization is dependent upon whether there is a reasonable degree of isomorphism with the situation in which the theory or research is to be applied. This might be ascertained by applying a set of criteria developed by Katzell[81] and described briefly as follows:

- Size — The number of interdependent organizational members is a crucial variable when attempting to transfer research findings from one kind of organization to another. A number of researchers have related employee absence and turnover, communication difficulties, job satisfaction, and morale to large size. While there is a need for more research on problems caused by size, it is clear that what holds for small organizations may not hold for large organizations.
- Interaction and Interdependence — This parameter deals essentially with structuring the work environment. Katz and Kahn, for instance, argue that where task interdependence is high, where creative requirements are minimal, and where identification with organizational goals is not required, a hierarchical authority structure calling for close supervision is indicated. Where the opposite circumstances obtain, a more democratic alternative should be used for maximum efficiency.[82]
- Personalities of Organizational Members — The motivations, abilities, and other personal qualities seem to be of paramount interest in determining policies and practices.
- Degree of Congruence or Disparity between Organizational Goals and the Needs and Goals of Members — Organizations vary according to goal internalization of their members. The extremes are illustrated by workers on assembly lines and scientists in research companies or universities.
- Who Has Ability and Motivation — As Katzell says,

> *. . . It would seem necessary to mold the organizational system so that it maximizes freedom of action and initiative for those who can and will take effective action, while eliciting compliance, support, or noninterference from others. Depending on the organizational loci of expertise and dedication, the appropriate work system may vary widely from situation to situation.*[83]

I find Katzell's guidelines useful in helping to determine whether a piece of research done in industry, for instance, is applicable to an educational organization. A set of rules might be devised to test the transferability of research findings. Another research task that needs doing is the actual testing of research findings together with the publication of findings that are applicable to particular types of institutions.

5. Not only should the assumptions underlying organization theory be examined and theories be built for sets of educational institutions, but they must be much more complex than are present efforts. The simplicity of present theories is one reason why they are of so little use to practitioners. New theories should give a much larger place to the environment, to networks of related organizations, to the goals of others than the executives, to unions, and, in higher education, to professors as managers. The theories should be closely related to the sociocultural context so as to be able to explain the place of women and minorities in organizations.

And, finally, some way must be found to rationalize or otherwise dispose of the criticisms of traditional organizational theories. The critics are not all aiming at the same aspects of organizational theory, and they do not agree with one another. Some intellectual framework must be found so that those criticisms that survive close scrutiny can be incorporated into a new paradigm.

NOTES

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14. Perrow, "Demystifying Organizations."
15. J. Wolff, "Women in Organizations," in *Critical Issues in Organizations*, ed. Clegg and Dunkerly, pp. 7-20.
16. D. E. Griffiths, "The Individual in Organization: A Theoretical Perspective," *Educational Administration Quarterly* 13, 2 (Spring 1977): 13.
17. There are, for example, more than 500 public college campuses now unionized as reported in J. H. Bunzel, "College Professors: Do They Have Managerial Status?" *The Wall Street Journal*, 29 September 1978.
18. Greenfield, "Theory about Organization," p. 76.
19. Bunzel, "College Professors: Do They Have Managerial Status?"
20. Yeshiva v. NLRB, 582 F2d 686 (Cir-2d 1978).
21. Pfeffer and Salancik, *The External Control of Organizations*.
22. Perrow, *Complex Organizations*, pp. 217-37.
23. J. K. Benson, review of *The External Control of Organizations*, J. Pfeffer and G. R. Salancik, in *Administrative Science Quarterly* 23, 2 (June 1978): 359.
24. Ibid.

25. Pfeffer and Salancik, *The External Control of Organizations*, p. 2.
26. Ibid., p. 2.
27. Ibid., p. 3.
28. Ibid., p. 12.
29. Ibid., p. 14.
30. Ibid., p. 15.
31. Ibid., p. 30.
32. Some examples are: School of Human Services, St. John's University; School of Human Resources and Education, West Virginia University; and New York University School of Education, Health, Nursing, and Arts Professions.
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Acknowledgments

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